

Inquirer (An)

14

OBSERVATIONS

ON A

LETTER

FROM DR. N. CHAPMAN, OF PHILADELPHIA,

TO

DR. W. B. TYLER, OF FREDERICK (MD.)

ON THE SUBJECT OF

CHOLERA, ✓

AS APPEARING IN

PHILADELPHIA, AUGUST, 1832.

"Is it not strange that medical men should for the first time have abandoned all their principles of practice, and throwing aside reason, catch at any shadow that was passing by, in the gloom of *deficient or erroneous* pathology"??!

"Remarks on the pathology and treatment of
the disease termed Malignant Cholera."
Am. Jour. of Med. Sci. for Aug. 1832. p. 533.

1833.

TO DR. TYLER.

SIR,

To no one can these pages be so properly addressed, as to you, to whom we are indebted for the appearance of the letter, which is the subject of the following remarks.—It is to be lamented, that you have not yourself (having seen the disease as I have heard) given us your sentiments on it, and thereby have enabled us to form an opinion as to your agreement or disagreement with the views of your correspondent.—After having waited nearly six months, in the hopes of seeing some observations on a publication so widely disseminated, from some practitioner to whom the disease had become familiar by the “lights of actual observation and experience,” I have thought it expedient to analyze the views therein expressed; and I have taken the same opportunity to point out various apparent inconsistencies, when tested by the former printed opinions of the writer of the letter; but which seem incontestibly to flow, from a due comparison of them.

I remain, Sir,

very respectfully,

AN INQUIRER.

COPY

OF A LETTER FROM DR. N. CHAPMAN, OF PHILADELPHIA,
TO DR. WM. BRADLEY TYLER, OF FREDERICK, (MD.)

[*Note.* This copy is from the Frederick Herald of August 25, 1832. The corrections, as appearing in the National Gazette of September 4, 1832, are given in italic, and between brackets, at the places thus corrected.]

My dear sir: I have delayed to answer your letter, till I had formed some decision as to the nature and treatment of the Pestilential Cholera which is now prevailing. These are points on which so much difference of opinion existed, that I found it impossible to make up my mind as to them, without the lights of actual observation and experience. I have now seen the disease sufficiently to enable me to arrive at satisfactory, and I trust, just conclusions on the subject. But I can present in the narrow compass of a letter, only a very concise and imperfect exhibition of my views, and indeed, such are my incessant occupations, that I have scarcely leisure to execute even this slight sketch.

The disease, *wholly independent of contagion*, is caused by an epidemic agency, of which we know nothing with certainty. It is not improbable, however, that it is owing to an æriform poison, which acting through the medium of the stomach on the ganglionic nerves, so impairs that system, that its functions are in a greater or less degree suspended. As always happens, where sensorial or nervous influence is withheld, there is in this case, a recession of blood from the periphery [*of the body*], and correspondent accumulations of it in the deep seated vessels, subversive of the proper distribution of it in the circulation, attended by a vitiation or suppression of the secretions. This, in a word, is my theory of the disease, the truth of which, I think, is sustained by the symptoms, the phenomena on dissection, and the mode of cure.

It is generally held here, that Cholera is pretty [*almost*] uniformly preceded by considerable disturbances of the alimentary canal, by nausea or purging, or the two united.—That affections of this sort, are very common in the city and elsewhere, cannot be denied. But whether they constitute the preliminary stage of the disease is very doubtful. It seems to me, they ought rather to be considered as a condition, arising from distinct sources of irritation predispo-

sing to the disease. Can it be credited, that a cause ultimately operating so powerfully as that of Cholera, should endure for three or four or five days, merely teasing in this slight manner, the stomach or bowels? The transition from these mild and lingering affections, to the explosion of Cholera in its fullest force, is far too sudden and violent, to suppose that they are one and the same disease, varied only by stages. I know not the analogies by which the hypothesis can be supported. Nor is this preclusive [*prelusive*] indisposition mentioned by any of the writers on Asiatic Cholera whom I have consulted. It is scarcely to be presumed, that so prominent and important a fact, had it an existence, could have possibly escaped the attention of these very able and experienced historians of the disease. Being attached to armies, and more particularly from their position in hospitals, they enjoyed the best, and peculiar advantages, for accurate and discriminating observations. It was first noticed and promulgated by some of the British publications, though not sanctioned by all, and from a similar coincidence [*of such derangements*] * of gastric and entiretic derangement with the epidemic in this country, the notion has been espoused by us. Be it as it may, such [*these*] disorders should at once be removed, as they are apt at all events, to invite an attack of Cholera. They do not differ from the ordinary complaints of the season, and require no peculiar management.

Genuine Cholera, for the most part, comes on with little or no premonition. The earliest symptoms are complaints of load, and oppression, and anxiety about the præcordia, with an internal sense of heat, referrible to the stomach or bowels, with great thirst and a whitish tongue, and at the same time, the head is confused, the expression of countenance haggard, accompanied by slight nervous tremors, muscular weakness, cool skin, and either a quick and somewhat feeble, or a full and struggling pulse. Copious evacuations upwards and downwards, of [*a*] fluid resembling dirty or turbid rice water, with flocculi mixed in it, soon occur, followed by cramps or spasms of the muscles of the extremities and abdomen. These are seldom so violent as has been represented, and never extend to the alimentary canal. An aggravation of the preceding symptoms rapidly takes place, and in half† an hour or more, the tongue becomes icy cold, the skin more chilled and sodden, though feeling hot to the patient, covered with a dewy viscid perspiration, the hands shrivelled or wilted, as if mascerated, the nails of the fingers blue, the pulse scarcely or not at all perceptible, the face sunken, especially the eyes, around which is a dark circle. This colour gradually diffuses itself [*more or less*] over the entire surface, partaking of the

* "*Of gastric and entiretic derangement,*" omitted in the corrected copy.

† "*Half,*" omitted in the corrected copy.

various shades of lividness, from a saturnine to a blueish or blackish hue. During this period, the thirst is intense, the heat of the stomach in some instances is increased to a burning sensation, the respiration greatly embarrassed, the air expired cold, the voice low, or whispering and plaintive, the diaphragm convulsed, and there is a total suppression of the urinary and other secretions. Discharges from the alimentary canal, and the spasms, likewise cease or are much diminished. [*The intellectual faculties, though obtuse, are seldom otherwise affected, and in some instances, their entire integrity is throughout preserved.*] Death ultimately takes place in a sort of tranquil stupor, or with indescribable jactitation and distress, the latter state being by far the most [*more*] common.

As I have briefly described the disease, such is the tenor of its character and progress, though occasionally diversified in some respects. Thus I have seen its accession as sudden as the electric shock, and have met with cases without spasms, or vomiting or purging. Many other anomalies might be mentioned, could I indulge in such details.—The disease may be properly divided in most instances, into two stages,—that of aggression, and collapse.

Called at the commencement of an attack, unless there is extreme depression, I bleed very* freely from the arm, and uniformly cup the epigastrium, and give calomel largely, combined or not with opium, according to the severity of the spasms. The case will almost invariably yield to these remedies, and we have no further trouble concerning it. But where the attack is confirmed, or in other words, the state of collapse exists, the difficulties of management are vastly increased, and the practice is somewhat different. The first step, under such circumstances, is to puke actively with tepid salt and water, a tumbler full at a time. This usually settles the stomach, allays thirst, produces some degree of reaction, a stronger pulse, increased warmth of surface, and a resolution of the spasms. Co-operating in the same design of arousing the vital forces, and exciting the skin particularly, the body and extremities may be rubbed with warm flannels. Let a vein be then opened, and if the blood flows freely, take a large quantity, and especially should the pulse rise and the blood become florid. But where the reverse happens, or you have slowly to coax out the blood, or the pulse is sensibly weakened by the loss of it, stop the operation, and apply twenty or thirty cups to the abdomen, including the epigastrium, which, though they may not draw much blood, are eminently serviceable as revellents. The cups are to be succeeded by a blister to the same parts. Calomel is next to be given in the dose of five, ten or twenty grains, frequently repeated, till the aggregate amounts to about a drachm, and then worked off with a table spoonful of castor oil.

* "*Very*," omitted in the corrected copy.

As the result of these means, there are commonly bilious evacuations, discharges of urine, and other proofs of the restoration of secretory power. Little more is demanded than what has been mentioned. I have, however, sometimes known, though rarely, that at this point of the case, irritability of the stomach to return, with the appearance of approaching exhaustion, in which event, stimuli are to be resorted to; the best of which are, a strong infusion of cayenne pepper, or clove tea, or the spirits of camphor, or the aromatic spirits of ammonia, or mint julep.—But they are cautiously to be administered, and in small portions, or they are instantly rejected, or they overwhelm the energies of life, or more slowly induce typhoid prostration.

Drink is sometimes vehemently solicited, particularly in the height of the attack, and the instinctive desire for cold water, or even for ice, may be gratified in moderation. The proper nourishment in convalescence, the only time when any is wanted or to be allowed, is chicken water or beef tea, rendered agreeably pungent with cayenne pepper. Thus I have hastily laid before you an outline of my mode of managing this terrible* disease. It may be observed that, with scarcely an exception, it is depletory or evacuant. Deluded by false appearances of debility in the disease, and still more by the weight of authority, I adopted when it first broke out among us, in common with my medical friends, a course of practice in conformity with such an impression—and most disastrous was the issue. Nearly every patient, amounting to five or six, [*in the different hospitals,*] died. The prominent indications seemed to call for heat to the surface, and the internal exhibition of the diffusible excitants. Every variety of bath, hot [*warm*] water, vapour, heated air and topical applications of hot sand, or oats, or salt, &c. were used, and also frictions with the spirits of turpentine alone, or united with camphorated mercurial ointment, and other articles. Brandy, ether, camphor, vol alkali, &c. &c. were in succession tried, and the whole of these means with no other effect, than an inconceivable exasperation. The suffering indeed, induced, was as great as I have ever witnessed from the application of any remedial process. No practical lesson is more important than that in the cure of this disease, all such appliances and medicines are mischievous, till evacuations are premised, and then to be most discreetly directed. [*The system previously is utterly intolerant of them, and I have found it better to expose the patient naked to cool air, than to cover him, even with a blanket.*]

It were easy to acquaint you with divers other methods of treating this epidemic, or to enumerate a number of special remedies that have been proposed. Dismayed, as it were, by the fearful charac-

* "*Terrible,*" omitted in the corrected copy.

ter of the disease, practitioners have been too prone in its treatment, to abandon their principles and well tried remedies, in analogous cases, to seek a resource in specifics and nostrums.

I do not mean to vaunt of my success, but on a fair comparison of all that I have seen attempted, I am led to an unqualified preference of my own plan [*of this plan.*] It cannot be charged with being tentative or empirical—is deduced [*on the contrary*] from established views of pathology and therapeutics, and is sanctioned in most of its features by the lengthened and concurrent experience of the distinguished and authoritative writers on the disease in India. Many may be cured by it, and some will sink under the force of the attack in despite of your efforts. The case not being too far advanced, a triumph over the disease is pretty certain. Cholera is, on the whole, more tractable than yellow fever, or the winter pestilence, which devastated our country during the late war.

Ever, my dear sir,

Yours, most truly,

N. CHAPMAN.

Philadelphia, August 18, 1832.

To Dr. Wm. Bradley Tyler, Frederick, (Md.)

[*P. S. I have omitted to mention that this very practice, by emetics, calomel, bleeding, &c. is the one which I have for twenty years taught in my lectures, as most appropriate to the worst forms of the ordinary cholera of our own country. You will find a tolerable synopsis of it in my work on the Materia Medica. I will only add, that there is considerable difference of opinion among us as to the best means of puking, some preferring ipecacuanha or the sulphate of zinc., &c. In Britain, the mustard emetic seems chiefly to have been used, while in France the ipecacuanha, and in Russia, and other countries of the north of Europe, the salt and water. The latter, on the whole, I think is most appropriate, though I have in some instances resorted to the ipecacuanha, very advantageously.*]

Date.	BUTTERWOOD. Dr. Brunkle.	CATHARINE st. Dr. Condie.	CHERRY ST. Dr. Meigs.	CHESTER st. Dr. Lukens.	CROWN st. Dr. Taylor.	DOCK st. Dr. Horner.	JONES' ALLEY. Dr. Parrish.	LOCUST st. Dr. Chapman.	LOMBARD st. Dr. Harris.	MARLBORO' st. Dr. Kline.	MOYAMENING. Dr. Thomson.	PENN st. Dr. Hodge.	LOMBARD & 6 Dr. Jackson.	RACE st. Dr. Harlan.	S. Third st.	ALMS House.	PRISON.	PRIVATE PRACTICE.
	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.	Cases. Died.
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31	0 0	2 2	1 1	0 0	0 0	0 0	1 0	1 1	0 0	0 0	0 0	0 0	0 0	0 0	0 0	0 0	0 0	5 2
Aug. 1	0 0	0 0	0 0	0 0	0 0	0 0	1 0	2 1	0 0	0 0	1 1	0 0	0 0	0 0	0 0	1 1	13 1	16 5
2	2 1	3 2	0 0	0 0	0 0	0 0	1 0	0 0	1 0	0 0	5 3	0 0	0 0	2 2	0 0	5 2	8 3	8 3
3	0 1	3 0	0 0	0 0	0 0	1 1	0 2	2 1	2 0	0 0	5 0	0 0	0 0	0 0	0 0	1 1	4 4	13 4
4	0 0	0 0	0 0	1 0	0 0	2 1	1 1	0 1	0 1	0 0	0 3	0 0	0 0	5 1	0 0	0 0	9 1	27 4
S. 5	2 0	0 0	0 0	2 2	1 0	3 1	1 1	2 0	0 0	0 0	15 4	1 1	1 0	0 0	0 0	11 6	0 0	26 10
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8	1 0	3 3	0 0	0 1	6 1	0 1	0 0	4 2	2 0	3 2	7 4	0 0	8 4	1 2	0 0	18 15	1 1	43 7
9	1 0	3 3	0 0	0 1	6 1	0 1	0 0	4 2	2 0	3 2	7 4	0 0	8 4	1 2	0 0	18 15	0 0	43 7
10	0 0	4 2	1 1	0 0	6 0	1 1	3 1	3 0	1 2	1 1	10 5	2 0	6 3	0 1	0 0	14 4	1 1	83 18
S. 11	1 1	4 1	0 0	1 0	4 0	3 0	5 0	3 0	1 0	2 0	3 2	4 1	5 1	3 3	0 0	7 2	0 1	76 14
12	2 0	5 2	1 0	0 0	2 2	2 1	1 2	0 0	6 3	4 1	6 1	1 1	5 2	0 0	1 0	5 4	0 2	66 12
13	1 2	8 1	0 0	0 0	0 2	0 0	0 0	2 1	2 4	0 0	7 1	3 1	1 3	4 2	0 0	8 7	0 0	94 24
14	0 0	9 2	1 0	0 0	3 0	1 0	2 0	4 1	0 1	0 0	6 8	2 2	0 1	3 1	0 0	5 7	0 0	70 12
15	3 0	0 3	0 0	0 0	5 2	3 1	2 1	0 0	2 0	1 0	8 4	0 1	1 0	2 0	2 1	4 3	3 0	36 6
16	2 0	6 1	1 0	1 0	2 2	0 0	0 0	2 1	1 1	1 1	7 4	0 0	3 1	2 1	0 0	1 1	1 0	62 14
17	2 1	3 0	0 0	2 0	2 0	2 1	4 1	1 1	2 0	0 0	8 5	0 0	4 1	3 0	2 0	0 1	0 0	49 11
S. 18	0 0	2 1	0 0	1 0	1 0	3 0	1 0	0 0	0 1	1 0	4 2	0 0	2 0	3 1	1 1	0 0	0 0	53 11
19	1 0	4 4	0 0	1 0	1 0	0 1	1 0	1 0	0 0	2 0	6 1	3 0	2 0	2 0	0 0	4 0	0 0	20 5
20	2 0	2 2	1 0	0 0	0 0	1 0	0 0	0 0	2 0	2 1	5 1	0 0	2 0	3 1	0 0	1 2	0 0	31 7
21	0 0	3 0	1 0	1 0	2 0	1 0	2 0	1 0	0 0	2 0	4 2	0 0	2 0	2 0	2 0	0 2	0 0	27 4
22	5 0	4 0	0 0	0 0	6 0	0 0	1 0	1 0	0 0	0 0	3 0	0 0	1 0	1 1	2 1	2 1	1 0	20 4
23	1 0	1 0	0 0	0 1	0 1	1 0	2 2	0 0	0 0	0 0	3 0	3 0	2 1	4 0	2 1	0 0	0 0	11 4
24	1 1	1 1	0 0	3 0	3 0	3 0	2 0	0 0	0 0	4 3	3 0	3 0	1 0	1 0	1 1	1 1	0 0	21 4
S. 25	1 1	1 1	0 0	0 1	0 1	0 0	0 1	0 0	0 0	2 0	0 0	0 0	1 1	2 0	1 0	1 0	0 0	14 4
26	1 1	1 0	0 0	1 0	4 0	0 0	0 0	0 1	1 1	8 2	0 0	3 0	3 0	0 0	0 0	0 0	0 0	7 1
27	1 0	1 0	0 0	2 0	2 0	0 1	1 1	3 0	0 0	4 1	2 0	2 0	2 1	0 1	0 0	0 0	0 0	5 1
28	1 0	1 0	0 0	2 0	3 1	0 0	0 0	3 0	1 0	3 0	1 0	1 0	0 1	0 0	0 0	0 0	0 0	5 1
29	2 0	3 0	0 0	3 0	1 0	4 0	0 0	3 0	0 1	3 0	0 1	0 0	0 0	0 0	0 0	0 0	0 0	7 2
30	0 0	1 1	0 0	1 1	2 0	1 0	2 0	1 0	1 0	1 0	1 0	2 0	1 0	2 0	1 0	0 0	0 0	8 2
31	0 0	4 0	0 0	4 0	1 0	4 0	0 0	4 0	0 0	2 1	1 0	0 0	0 0	0 0	0 0	0 0	0 0	10 3
	28:8	88:33	8:2	19:6	71:18	25:12	43:11	54:20	31:14	27:8	179:85	35:8	65:22	71:30	21:6	117:82	77:37	1133:236
	1 to 3½	1 to 22.3	1 to 4	1 to 3.16	1 to 4	1 to 2½	1 to 4	1 to 26.10	1 to 21.20	1 to 33.8	1 to 2.10	1 to 43.8	1 to 3	1 to 2½	1 to 3½			

From July 27 to Aug. 4	WEEKLY REPORTS of each HOSPITAL. Some errors in Alms House and Prison Reports.																	
Aug. 4	2 2	8 4	1 1	1 0		3 2	4 4	6 5	3 1		11 7			7 3		7 4	26 6	69 18
5 to 11	7 3	17 9	2 1	12 6	31 9	9 6	10 2	22 7	13 3	14 5	74 42	15 2	28 12	26 18	8 2	78 49	47 31	323 66
11 to 18	10 3	33 10	3 0	4 0	15 8	11 3	10 4	9 4	13 10	7 2	46 25	6 5	16 8	17 5	6 2	23 23	4	430 90
19 to 25	9 0	17 7	2 0	2 0	12 2	2 1	8 0	7 3	2 0	6 1	27 7	9 0	13 2	15 2	7 2	9 6	1	144 32
26 to 31		5 1			13 1		11 2	10 1	0 0		21 4	5 1	8 0	6 2				42 10

Two or three hospitals are not included, being open, but for a very short time.

TO DR. N. CHAPMAN.

SIR—A considerable period has elapsed since the appearance of your letter to Dr. Tyler, on the subject of Cholera.—Published first in the Frederick-town Herald, and having with various encomiastic remarks, been transplanted into most of the papers of the union; it has consequently become public property, liable to be remarked on by any individual who may deem it worthy of consideration; either, as relating to the disease of which it professes to treat, or to those discrepancies which it displays with other parts of your accredited writings.

I have awaited, Sir, to see from some professional pen, either in Philadelphia or elsewhere, such observations, which it appears to me, flow naturally from the perusal of your letter, and from a comparison of its sentiments, with your well known opinions elsewhere given. None such having appeared, either from an acquiescence in its contents, or from some other motive, I have at last concluded to give your letter that examination, which the importance of the subject demands; emanating as it does, from authority deemed so high, and rendered still more important, when it is remembered that you fill the responsible situation of Professor of the theory and practice of Medicine, &c. in the oldest of the Medical Institutions of our country.

I trust Sir, to your love of medical science, and of your encouragement to a free inquiry in every department thereof; together with your liberal promotion of every investigation, even into your own particular views, to obtain my excuse for the remarks I may be called on to make, and the inductions to be drawn, in the course of my investigation into the merits and bearings of your letter.

Placed in the elevated station above adverted to, the medical profession naturally looked for your views respecting that wide-spreading disease, which, within the short period of six months has carried, and still is carrying desolation and dismay throughout our continent, after fifteen years' ravage of the European and Asiatic world.—Such a length of time would appear sufficient for any common observer to have studied fully, the character, peculiarities and nature of this disease, from the numerous works printed on the subject, in every place where it has found a footing.—Embracing nearly half the time of your professional career, how is it, that, (unaccompanied by the terrors of its actual presence,) its visitation

had not been prepared for; nor its investigation pursued, so far as I have been able to learn, by any of the practical professors in our numerous schools of medicine?—For, considering its ravages elsewhere: seeing that it had surmounted rivers, seas, deserts, mountains, forests, &c.—that it had been equally fatal in the coldest and hottest climates; we surely, had every reasonable ground to anticipate a closer connexion with it; and consequently, had both time and interest to engage in its pursuit, and prepare for its visitation. —But, when it actually reached our shores, how were we prepared? Until its sudden explosion in Canada, (however originating there,) we scarcely knew it except by name. All were in the dark, since even the experience of others seems to have been but little thought of.—Nay, its rapid strides in Canada, and even its advance to the city of New York, seem scarcely to have awakened the members of the profession in Philadelphia, to the full danger that pressed upon them, beyond that which was felt by the affrighted community at large; and which looked to the profession in the day of trial and distress, but found it nearly as uninformed and unsettled as itself, in a concern of such *vital* importance.—The Medical Committee, after a three weeks' absence, and a close observance of the disease in Canada, brought back but little consolation; nor was their promised pledge of further information withdrawn, until the pestilence had nearly left your city. Such too, was precisely the fact as to *your letter*, which it seems indeed, was intended merely for the private information of your correspondent. —The disease brushed by you with rapidity; and by the time your letter did appear, Philadelphia had sunk into a state of comparative tranquillity. Certainly, this did not arise from medical acumen or professional skill; for all were paralyzed, and scarcely knew what direction to pursue, amid the conflicting tides of opposing notions, both in theory and in practice, that were continually promulgated.—In private and in hospital practice, it would seem, so far as I can learn, as if no uniformity prevailed; and the mortality, proportioned to those attacked, was, it is believed, nearly as great as in other places.

The letter I am about to analyze, is dated on the 18th of August, 1832, three weeks after the full breaking out of the disease, and the opening of the hospitals in your city, on the 27th July, according to the newspapers.—It appeared in the Frederick-town Herald on the 25th of August, and in the National Gazette of Philadelphia, on the 1st of September, *at which time your hospitals were closed*—so that, in fact, it was absolutely useless to the community!—Why it was not *directly* addressed as a circular to the profession in Philadelphia, the immediate seat of the disease, and of discordant practice, rather than to Dr. Tyler, (in a region not then affected, but to which it would have duly arrived,) containing, it would seem,

matter deemed of high importance, and of *immediate* utility to your fellow citizens, I cannot learn or comprehend. If the common saying, of charity begins at home, ever could be properly applied, it would indeed seem connected with the then existing state of Philadelphia—and the benefit of your lucubrations might have been profitably exhibited, where it was urgently required; and nearly two weeks of desolation and woe might have thus been prevented! At the actual time of its *circuitously* reaching your city, its importance was nugatory; the disease had nearly subsided, the hospitals were closed, and death had obviously sheathed his sword of destruction. The loss, no doubt, was great, in thus being precluded from testing at the time, the validity and perfection of your “own plan” of treatment; and we must remain satisfied (unless a not-improbable recurrence of the disease takes place) with the statement you have thus so concisely and indirectly afforded.

It has been reported, that you do not assume to yourself the credit of the treatment you so warmly recommend. I cannot well reconcile this report, I confess, to the general tenor of your letter, in which we find the expressions, “the outline of *my mode* of managing this terrible disease,” &c.—“my own plan,” &c.—which, altho’ modified slightly in the *corrected copy*, as it is called in the National Gazette, yet we cannot doubt your maintaining it absolutely as your own;—for how otherwise could you venture to speak of not “*vaunting of their success*”? It is indeed true, that very numerous are the writers who have preceded you in the details of the theory, symptomatology, treatment, &c. of this disease, not only on the shores of Hindostan, but in every part of Europe in which it progressively appeared; as well as before and since it reached the Canadas and the United States.—Should it be affirmed, that five hundred treatises have appeared upon it, it would probably be within due bounds. Still however, since your conclusions as to the nature and treatment of the disease, are founded on the “lights of (your own) actual observation and experience,” presenting at the same time only a very concise and imperfect exhibition of your views, it seems conclusively to follow, that all that is thus imperfectly detailed, is considered as your own, and as such, promulgated, for the consideration of your correspondent, if not for that of the public at large.—Fearful nevertheless, of misapprehending your real views in these particulars, I have thought it expedient to give an accurate copy of your letter as it first appeared in the Frederick Herald, and also, to mark the corrections and additions of that which appeared in the National Gazette; in order to enable every one to judge of the importance of those corrections, and how far they really help to modify or alter the conclusions which the first would naturally elicit.

In the subsequent remarks, I shall endeavour to demonstrate, that the depletory system was most accordant with the principles you

at first abandoned; for it had been recommended by a large proportion of the writers on the subject: if therefore, it was your misfortune to have been led astray by circumstances to be adverted to; and if your good genius happily led you back to well tried principles and remedies, they were the “principles and remedies” of other writers of at least fifteen years’ notoriety.

In adverting to Cholera as an Epidemic, allow me to call your attention to the writings of the venerable and illustrious Rush, whose pages at present too greatly neglected, will hereafter attract that (renewed) notice for which they were formerly unrivalled in this country, whilst others of present notoriety will sink into oblivion.—As you have written largely on Epidemics, I would suggest for consideration the following remarks, and whether they have not been amply verified by the Epidemic that you describe in your letter to Dr. Tyler?

“From a short review of these facts, (says Dr. R. p. 174, &c.*) reason and humanity awake from their long repose in medicine, and unite in proclaiming, that it is time to take the cure of pestilential epidemics out of the hands of physicians, and place it in the hands of the people.”——“I would as soon believe, (p. 177) that ratafia was intended by the author of nature to be the only drink of man, instead of water, as believe that the knowledge of what relates to the health and lives of *a whole city or nation*, should be confined to one, and that, a small or privileged order of men. But, what have physicians, what have Universities or Medical Societies done, after the labours and studies of many centuries, towards lessening the mortality of pestilential fevers? *They have either copied or contradicted each other* in all their publications.”

I would also further recommend the following judicious remarks of the same enlightened physician, as to the precursory or premonitory symptoms, which, says he, it will be necessary to publish an account of, to prevent the formation and mortality of this (yellow) fever: and “to exhort the people, as soon as they feel those symptoms, to have immediate recourse to the remedies of purging or bleeding. The danger of delay in using one, or both these remedies, should be inculcated in the strongest terms; for the disease, like Time, has a lock on its forehead, but is bald behind.”(p. 177) It is true, Sir, Dr. Rush is speaking of the Yellow Fever:—but, were he now living, his towering and expanded mind would have urged the like, or stronger evidences of the premonitory symptoms of Cholera, and of the absolute importance of attending to them.

Very respectfully, I remain, Sir,

AN INQUIRER.

* Med. Inq. & Obs. Vol. III, 4th edition, 1815.

OBSERVATIONS
ON A LETTER ADDRESSED TO DR. TYLER,
ON THE SUBJECT OF THE
CHOLERA,
AS APPEARING IN PHILADELPHIA, IN AUGUST, 1832.

“MY DEAR SIR:—I have delayed to answer your letter, till I had formed some decision as to the nature and treatment of the Pestilential Cholera which is now prevailing.”

It is to be regretted that we have not a copy, and the date of Dr. Tyler's letter, to which this of August 18th, is an answer.—When it is remembered that the disease of which it treats, had scarcely been three weeks in Philadelphia; a knowledge of these dates might have enabled us to judge of the rapid decision of the writer as to its nature and appropriate treatment, founded on his own experience, between the 27th July and the time of writing; during which period, by the *returns of the Board of Health*,* 37 patients had been received in the Locust st. hospital, and 16 had died; that is, about 45 per cent., admitting the whole number received to be *actually and bona fide Cholera patients*; for, it is remarkable, that a gentleman who gives some account of the disease, and speculates deeply on it, has stated returns of a somewhat different nature—“Before concluding these remarks, I will insert from the Day Book the comparative results of the practice pursued, under the direction of the physician-in-chief, in the hospital, *prior to the adoption of my practice, and of that which I introduced*, upon the principles before explained. From July 27th to August 6th, 14 cholera patients were received into the hospital; of these, five were cured,† and nine died. From August 7th inclusively, to August 21st, twenty-eight cases occurred; of these, twenty were cured, and eight died. Both computations have been made, by excluding *all cases not choleric*; and in each, *some of the cases* were moribund, when received.”—By the report of the *Board of Health*, however, it would appear,

* See *Tables*, from the Philada. *Daily Chronicle* of Novem. 29, 1832.

† “Including,” says the writer in a note, “*two cured by my treatment, being my two first cases.*”——This practice then, must differ from that of Dr. Chapman, for both speak respectively, and positively of *their particular practice*. This is indeed rendered certain, from the previous part of the sentence itself, as above quoted. It is to be regretted, that we are not told, *how many were not cholera*, and *how many were actually moribund* when received, as well as *how long* such moribund cases lived, after reception. Vide *Am. Jour. of Med. Sci.* August, 1832, p. 541.

that from the 27th July to August 6th, *only* twelve patients were admitted, and seven died: and from the 7th inclusive to the 21st, of twenty-eight received, nine died.——There is some discrepancy, it will be perceived, in the reports of the Board of Health and of the gentleman alluded to, which it might be useful to reconcile. Perhaps no place is more appropriate than this, to refer to the condensed *tabular view* * of all the cases received and the deaths noticed during the month of August, in the *different hospitals*, as drawn up from the reports of the Board of Health: and which are derived from the daily reports afforded from each hospital respectively. If in this, any mistake occurs, now is the time effectually to remedy it.—We may here be permitted to enquire the reasons for preferring the name of “*pestilential Cholera*,” to any other of the numerous synonymes that have been given to the disease?—As we must have a name, it may be as well to be able to assign some reason for our nomenclatural distinction.

“These are points on which so much difference of opinion existed, that I found it impossible to make up my mind as to them, without the lights of actual observation and experience.”

This is all well enough, and will be adequately comprehended by the profession, so far as it is calculated to lead to a due conviction, of the earnest endeavours by “actual observation and experience” to come to “just conclusions”! But, admit for an instant, the conclusions drawn to be fully legitimate, and decisive to the mind of the writer; do we not perceive, that as these present views are different from those at first acted on, but which were doubtless, then, considered as conclusive; do we not perceive, I repeat, that, as you ask for yourself, personal observation and experience, (independently of all, who have preceded in their inquiries *during fifteen years*, on the subject of Cholera;) and that you have required *only fifteen days*, to bring you to the very antipodes of treatment and of theory; it is highly probable, every other man not disposed to swear *in verba magistri*, will claim an equal privilege of delaying to make up his mind, until equally sharing in the “lights of actual observation and experience.”—In thus rejecting a decision on the opinions of others, especially when *guarded* by long established principles, taught so long, and constantly practised on; we perceive a determination, to acquire information, only from the narrow boundaries of our own limited comprehensions! The experience of others, *even when abetting* our own long tried principles and remedies, are inadequate to enable us to “make up” our minds

* See *Tables*.

without being fortified by self-experience!—Let us suppose, as must be admitted to be sometimes the case, *such* asserted self-experience, to lead to a false judgment or conclusion; or, that the judgment, though correct, has been founded on a *false experience*, or incorrect observation! What then? Have persons never been known to think differently of diseases and remedies at different times? and yet found such diversity of conclusions on an affirmed experience in both cases; *one of which*, if not both, obviously must be the result of error in judgment from hasty and incorrect observation? Now, which of the two shall the profession admit, on the recommendation of the same person? the opinion primarily maintained, and ultimately disavowed; or a later one, which has no stronger foundation than *an experience* thus admitted to be vacillating; and acknowledged as too hastily adopted, and considered as correct?—Experience then, may mean any thing or nothing; for if one day's experience is set at naught by that of the succeeding, who can be sure, that the judgment or conclusions drawn from this, shall not in a short time be found to be equally incorrect!—Certainly then, if we pay no respect to the experience of others; or at least, find “it impossible to make up our minds” when they differ, we must be under a strange delusion, should we reckon on the confidence of the medical profession in our own; and more especially when that is acknowledged to have been at first led astray, by a departure from old established principles, and practice!

“I have now seen the disease sufficiently to enable me to arrive at satisfactory, and I trust, just conclusions on the subject. But I can present in the narrow compass of a letter, only a very concise and imperfect exhibition of *my views*, and indeed, such are my incessant occupations, that I have scarcely leisure to execute even this slight sketch.”

How Sir, will it be with others? Will they have no scruples on the subject of “satisfactory conclusions”? May they not possibly doubt their being “just,” when they are so diametrically the reverse of those you set off with? Is the last conclusion so absolute, that no one shall venture to adhere to the first? Conclusions drawn by others, were to themselves, doubtless, as satisfactory and just, as yours have appeared to you.—These have been opposed, and opposite indications drawn from the same train of symptoms! Now, as the *evacuant plan* met with opposition, even from yourself at first; this present testimony in its favour, will probably not alter the opinions of those who are waiting to test it by “actual observation and experience,” from seeing such difference of opinion maintained, not only by different individuals, but by the same person at different times! And if, by a comparison of the mortality at the differ-

ent hospitals, it is found that this was not very different by any particular plan, or by a change of practice from the *stimulating* to the *evacuating* plan; no great reason will be perceived from the facts, why each individual should not, in justice to himself, await his own self-experience and observation, therefrom to draw conclusions. Unacquainted with the particular circumstances of the practice, &c. of the different hospitals, I can of course, say nothing, except that I trust, the able physicians placed at their heads, will, respectively afford us information and explanation, on many particulars of high importance and interest; on which the profession are altogether undecided. Indeed, they seem absolutely called upon to vindicate themselves, from the *implied* want of success, which your words convey, at a later part of your letter; and thus enable us to estimate accurately the claims of superiority therein maintained!

You tell your correspondent, that you can present in the narrow compass of a letter, only a "*concise and imperfect* exhibition of your [*my*] views;" and afford subject of sympathy under those labours, and those incessant occupations, which left you "scarcely leisure to execute even this slight sketch."—Concise indeed, and imperfect, we will agree to call it with you; trusting, that hereafter we shall have the *full benefit* of your experience, derived from ample, extensive and accurate *clinical* observations on the disease, in all its various forms and stages. Let us no longer have the invidious questions proposed, "In the four quarters of the globe, who reads an American book?—or what does the world owe to American Physicians or Surgeons?"—Let us have a work deserving of immortality, on a disease that has rendered so many immortal; but which yet wants the finish of satisfactory and just conclusions!

Let us here be permitted to advert to the labours of Dr. Rush, during the eventful period of the Yellow Fever of 1793, in the months of July, August, September and October, and compare them with the "*incessant* occupations" which surrounded you from the 27th July to the date of your letter.—In this disastrous *period of three weeks*, we see by the table, about *three dozen* patients had exercised your skill, or nearly two a day; of which, if all were *real* Cholera cases, 16 died, or about 1 in every $2\frac{1}{2}$ cases!—A comparison of the other hospitals, either collectively, individually, or by weekly estimate, will show an average mortality scarcely superior.—But the labours of Dr. Rush also, are not the less deserving of recollection in the minds of Philadelphians; nor the account of *his* avocations, and *his* occupations, to which he devoted himself, in that afflictive and heart-rending period of *four months*!

"Between the 8th and 15th of September,"* says this admirable man, "I *visited* and prescribed for between one hundred and 120 patients *a day*! Several of my pupils visited one-fourth or one-

* Account of the Bil. Yellow Fever of 1793, p. 182, *Med. Inq. & Obs.*

fifth part of that number. In the short intervals of business, spent at my meals, my house was filled with patients," &c.—"I went to bed in conformity to habit only, for it ceased to afford me rest or refreshment. When it was evening I wished for morning; and when it was morning, the prospect of the labours of the day, at which I often shuddered, caused me to wish for the return of evening."

On the evening of the 14th of September, Dr. Rush was attacked with the disease, requiring bleeding and other evacuations; yet he admitted persons to his chamber next morning for advice; and on the 16th prescribed in his parlour "for not less than 100 persons."—On the 19th, resuming his labours abroad, though so weak, as with difficulty to ascend a pair of stairs. For 5 or 6 weeks, not less than from 50 to 150 received advice at his house, independently of his external duties; and "the evening did not bring with it the least relaxation from my labours. I received letters *every day* from the country, and from distant parts of the union, containing *inquiries into the mode of treating* the disease, and after the health and lives of persons who had remained in the city. The *business of every evening* was to answer these letters, *also*, to write to my family," &c. &c.

I forbear!—I should but transcribe the whole of Dr. Rush's interesting and important treatise on the Yellow Fever of 1793, were I to give a *full and adequate* idea of *his* incessant labours and occupation, both of mind and body; and that, in relation to a disease, *then*, as little, or perhaps less known to us, than the Cholera here treated of.—It will be allowed, that it was a period to try men's souls; beyond all other epidemics that have appeared amongst us;—a perfect acquiescence at that time in its *absolute contagious* character, among medical men, both here and elsewhere, rendered it doubly dangerous.—A doubt of the contagious character of Cholera, stripped *it* of half its terrors; so that I may venture to say, compared with the Yellow Fever of 1793, the Cholera of this season sinks into insignificance! Perhaps, it may be permitted me to apply to the deeds of your most illustrious predecessor in the practical chair, the words of the poet, "*Hic Labor, hoc opus est.*"

Before I conclude my observations on this sentence of the letter, allow me to ask, as you use the expression, "*my views*," whether they are claimed as *exclusively* your own; and as such, indicated now, for the first time?—for I perceive them, both in the original, and in the corrected copy.

"The disease, *wholly independent of contagion*, is caused by an epidemic agency, of which we know nothing with certainty. It is not improbable, however, that it is owing to an æriform poison, which acting through the medium of the stomach on the ganglionic nerves, so impairs *that* system, that its functions are in a greater or less degree suspended."

How wonderful must be the fact that could, in the short period of three weeks, and by the attendance on three dozen cases, by self-experience and observation, thus rapidly attained, (for all others are rejected) be qualified so decisively to cut the gordian knot, that has, for fifteen years, divided the writers on Cholera, and on which they are not yet agreed; that, even *in italics* the certainty is proclaimed, that “the disease is *wholly independent* of contagion”!—*Certain now*, of this important fact, the greater disappointment we must feel, by the confession made, that although caused by an epidemic agency, “we know nothing with certainty” of that agency!—“*Hinc illæ lachrymæ!*”—At least we have here a truism, not very consoling to the march of intellect in our profession; that during a lapse of twenty-two centuries since the æra of Hippocrates, we have attained so little light upon the subject!—If this be truly the case, why hazard so dogmatically, the affirmation of its non-contagious nature?—For my own part, I mean not to contest the point, which, like yourself, I do not credit; yet, with the numerous facts, &c. as yet not fully elucidated and explained, which strongly favour the doctrine of contagion, I should deem it at least prudent, merely to state it as an opinion, but would not chuse to affirm that opinion as absolute and certain.—Individual opinion is of little importance; and will probably be received only for what it is worth: it is certain, that neither your assertion nor mine, would satisfy the minds of those who hold an opposite opinion. They call for facts, and not opinions, to verify a circumstance of such primary importance! and in the assertion thus *unhesitatingly* advanced, I see nothing either demonstrative of, or negating the contested point.—I await, in common with your medical brethren, your full elucidation of the subject; which your extensive observations will enable you to demonstrate.

Admitting however, that we know nothing about this agency; it may perhaps be conceded, that a few facts connected with the subject of Epidemics, are to be found dispersed throughout the works on Medicine, from the time of Hippocrates to the present date; which, if *accurately* collected together, would probably, form a body of doctrine of infinite value. I am sure I need not point them out to you, who have written so learnedly on the causes, phenomena and laws of Epidemics!—You will agree with me nevertheless, that if we know but little as to epidemic agency, we are not much more advanced or decided on the subject of contagion. The precise distinction between it and infection, if any, is inadequately comprehended; and terms are employed perpetually, at variance with the apparent intentions of their authors: the contagious or non-contagious nature of Yellow Fever, is not even yet universally agreed on, amongst us; and with all these, and other particulars that will probably occur to your mind, perhaps you will

excuse me for saying, a *less authoritative* tone would have been more becoming.

Whatever promptitude of opinion or decision you assume, *on the doubtful point of contagion* above noticed; and admitting at the same time, that we *know nothing* of the epidemic agency producing Cholera, with certainty; in the succeeding clause of the sentence, we find a recurrence to *mere probability* as to its nature! I would have here preferred the words, not *impossible*.—If it is probably an “æriform poison,” as suggested, (though this suggestion is European,) do you not, from your chemical knowledge, think it also *probable*, that, as an æriform, and consequently a tangible substance, some idea would, ere this, have been acquired respecting it? You are well acquainted, no doubt, both with the simple and the compound gaseous fluids, that have within your own time, been successively discovered; you know the mode of procuring them for chemical research; their analytical detection and all the minutiae, by which they are distinguished from each other.—Which of all these, do you imagine the probability will rest on, in proof of your supposition? You also know that some appear poisonous positively, whilst others seem destructive, only negatively. But with all we do actually know, permit me to ask, if your probable case is strengthened in the slightest degree, by all the eudiometric experiments and observations that have been so numerous, both here and elsewhere? Have they pointed out any positive fact in any way fitted to elucidate the mysteries of miasmata? What, in short, has the Science of Medicine acquired in this respect from Chemistry?—If, however, you suppose this æriform poison to be *sui generis*, why did you not institute a series of experiments to determine it? How is it, that Dr. John Davy, so many years back, discovered (in the E. Indies) that the air *expired* in this disease, was not only cold, but greatly deficient in the amount of carbonic acid, which should have accompanied it; indicating a source of undue *spissitude* (excuse this old Boerhaavian term, which will probably be renewed in its application,) to the blood; how is it, that this ingenious chemist, neither saw, nor supposed it probable from his personal experience, and chemical experiments, that an æriform poison, untangible, and unknown, was the cause of Cholera? He was probably too considerate, to think of placing self-speculation, where fact only should be seen.—I fear Sir, the omission to verify by chemical experiment, this æriform poison, will preclude that immortality which so splendid a discovery would have insured:—and, that it will but lead us to regret, lest it should have been seen merely as a deceitful vision, of which we may say, with Macbeth—

“I have thee not, and yet I see thee still,”

———“a false creation,

Proceeding from the heat-oppressed brain,” &c.

At all events, as you are not certain of the fact, but merely regard it as probable, we shall pass its further consideration—indeed, we might the balance of the sentence; inasmuch as *its* probability rests entirely on that of its precursory cause, the æriform poison above mentioned. Admitting this unknown poison however to be actually conveyed to that great centrum sympathiæ, the stomach, and then to act through its intermedium on the ganglionic nerves, as several preceding writers have also asserted; I would be gratified to know, on which of these, or if on all? Perhaps, even a brief account of them, would aid us in this hypothetical description; especially, as a definition or description of terms, will very often prevent misapprehension.—Now, I am of opinion, that a want of this precision as to terms, has led to infinite confusion in every branch of medical science! and I would fain see it obviated, in the further inquiries that may be made as to this pestilential Cholera.

Permit me to observe, that I find the sentence rather obscure; and that I cannot determine, what system is referred to, as being suspended in its functions: whether it is the system of the stomach, or of the ganglionic nerves.—I should be gratified in learning also, whether this æriform poison is direct, and prompt; or rather slow and chronic, if I may say so, in its action? the former, I presume, by sympathy, will better explain some of those cases which we are informed are as sudden as the electric shock;—the latter view may harmonize better with those that are preceded by premonitory symptoms.—As in a path so little unravelled as is that of Cholera, we need every probable and possible means to reach our destination; so these ideas, like those in the letter, being estimated for no more than their worth, may very properly be regarded as more allied to hypothesis than to fact.

“As always happens, where sensorial or nervous influence is withheld, there is in this case, a recession of blood from the periphery, and correspondent accumulation of it in the deep seated vessels, subversive of the proper distribution of it in the circulation, attended by a vitiation or suppression of the secretions. This, in a word, *is my theory* of the disease, the truth of which, I think, is sustained by the symptoms, the phenomena on dissection, and the mode of cure.”

Admitting the truth of the proposition of the first part of this sentence, viz. that it “*always happens, where,*” &c. &c. we cannot Sir, surely forget, that the *premises* are, as yet, altogether unsettled, and dependent on your assertion alone! You recollect no doubt, that you have ventured to maintain, only the probability of the cause by which this nervous influence is withheld: its *certainly* therefore is problematical, even by your own admission!—Yet on so treacherous a basis, is erected a towering hypothesis; which,

in the succeeding sentence you pronounce “in a word” to be *your theory* “of the disease”!—How this problematical, æriform poisonous cause acts; at what point of time after reception, the “recession of blood from the periphery” ensues; with its corresponding accumulation in the deep seated vessels; all this, and more that seems intimately associated, is left unexplained. Although you thus give it to the profession as *your theory* (par excellence), you do not favour us with the smallest insight as to the catenation of cause and effect.—If we wish to comprehend it, here it stands, to be unriddled by any one who can dive deep enough to reach its aforesaid basis. But, as the onus probandi rests with the author, we may hope that in due time we shall be favoured with a complete development of your views, both on this especial point, and on the vitiating of the secretions; which, as I understand it, has a slight approximation to humoralism; and into the boundaries of which, you have hitherto opposed so determined a resistance.

In tacitly admitting this, for the present, as *your theory* of the disease; we reserve nevertheless the right of shewing, when you further advance your claim, that others have *preceded you* in the same opinion; nay, that it even seems to be unhesitatingly advanced as *his* also, by one of the subordinate attendants on your own hospital! It would embrace too much space at present, and besides, I would rather await the due presentation of *your* claim in its behalf, strengthened as it will be, by every possible proof!—I will barely ask in the interim, to what class are we to refer it? to solidism, or humoralism; or to a combination of both?—It will be time enough hereafter to examine whether it is fully “sustained by the symptoms, the phenomena on dissection, and the mode of cure.”

“It is generally held here, that Cholera is pretty uniformly preceded by considerable disturbances of the alimentary canal, by nausea or purging, or the two united. That affections of this sort, are very common in the city and elsewhere cannot be denied. But whether they constitute the preliminary stage of the disease is very doubtful.”

’Tis well! That such affections were common, you seem unequivocally to admit; though you hesitate to call them *premonitory symptoms*; nor will you even allow them to be the preliminary stage of the disease; at least, to you, it is very doubtful.—Nevertheless, all must be aware, that those who have had more ample experience, do affirm the fact to be so; of which we may find the most ample proof in the excellent compend of Boisseau, and in the Report of the Massachusetts Medical Society, both of them printed anterior to the Cholera appearing in Philadelphia. In short, there is scarcely a writer on Epidemics, including yourself, that

does not confirm the fact, that they invariably make all concurrent diseases assume their livery; and evince thereby, in these preliminary symptoms or premonitions, the necessity of regarding them as giving a powerful tendency to the complaint.—Thus, says Hillary in his “Inquiry into the means of improving medical knowledge,” Ed. 1761, p. 348, (a work by the bye, well deserving of recommendation,) “Thus, when a catarrhus fever is epidemical, how many people are affected with coughs, and a slight catarrh, yet go about their business, and are tolerably well. And, when a Diarrhoea febrilis, or a dysentery, are epidemical, how several people will have a loose belly, or a few loose stools, or a little griping, yet go about business and are well,” &c. It is true, you admit the fact, but endeavour to disprove the connexion which the so called premonitory symptoms, unquestionably have with the full formed disease.

“It seems to me, they ought rather to be considered as a *condition*, arising from distinct sources of irritation predisposing to the disease. Can it be credited, that a cause ultimately operating so powerfully as that of Cholera, should endure for three or four or five days, merely teasing in this slight manner, the stomach or bowels.”

We will admit the fact to be as you here affirm, for argument sake; yet, if they were so universal “*a condition*”^{*} as we are led to believe, both here, as well as in Europe and Asia, during the prevalence of the Epidemic; it seems difficult to deny some intimate connexion with it; or not to see in them, those premonitions, which, if unattended to, will soon run into the actual disease; and hence the strict propriety of forewarning the public, of the danger they incur, of lightly regarding them on any occasion.—As for the proposition of “Can it be credited,” &c. It may be asked, Why not?—Because one cannot, is his credence really so overwhelming, as to

* We are very glad to find you are *not singular* in this opinion, as the following extract may shew, and which, considering the works on Asiatic Cholera consulted, it seems extraordinary should have been overlooked. You will find it in the Madras Report on Cholera—or in Johnson’s Medico-Chirurgical Review, p. 609 of his No. for April last. The language, it will be perceived, has a great analogy with yours.—

“This most formidable disease does not appear to be attended by any premonitory symptoms which can be regarded as being at all peculiar to it; on the contrary, we may safely assert, that it is of sudden invasion: for though a slight nausea, a laxity of the bowels, and a general feeling of indisposition are often found to precede Cholera, yet these symptoms are evidently common to many acute diseases; and they are especially frequent in this climate without being followed by any graver ailment. When such symptoms are found to precede Cholera, they might with more truth, be regarded as indicating merely a certain deranged state of the alimentary organs, a *condition* of the body which certainly predisposes a person to an attack of Cholera.”

be the sole rule of faith with every one else, without previously attending as you have done, to self-experience and observation? If, as on a preceding occasion, the word *here* used had been "*improbable*," it might have passed; but, by the present formula, whoever ventures to admit the position combated by you, must inevitably be delivered up, as heretical in his opinions!—But what is meant precisely, by the sentence itself? You here appear to speak of Cholera, as if you absolutely knew its cause, but which you have explicitly disavowed. If then, "a cause operating so powerfully as that of Cholera," is positively unknown, why will you undertake to limit, and thereby undervalue its powers of action?—Surely, the greater action involves the lesser; and, if this cause, whatever it be, can thus promote the higher grade of the disease, it must, a fortiori, be able to promote all those preceding slighter "teazings" of the stomach and bowels! I wonder that any difficulty should present itself in this particular; when we know so well the power of *sympathetic action* in the stomach and bowels, in promoting good or evil in the highest degree! At all events, whatever may be individual credence, it will not alter the matter of fact, so ably and so extensively affirmed by hundreds of persons, conversant with the disease in every place, not for weeks only, but for months and years.

"The transition from these mild and lingering affections, to the explosion of Cholera in its fullest force, is far too sudden and violent, to suppose that they are one and the same disease, varied only by stages. I know not the analogies by which the hypothesis can be supported."

Admitting an unknown "æriiform poison" as the cause of Cholera, must it not, in its action on the stomach, &c. take some time (varying in different constitutions, &c.) to produce a train of effects from the lowest degree, to the "fullest explosion"? or is it in all cases as rapid as electricity?—Will not its action *probably* be resolved by most persons, (whatever it may be in itself,) into something like that which governs, or is produced by the miasmatic causes of intermittent and other febrile affections? Surely, little as we know of these miasmata, we may find strong "analogies" between them and the causes of Cholera.—Nay, so closely are they united, that the *Intermittent Choleræ** reported by Hoffman, are amongst the most interesting cases in his instructive writings. Torti has also favoured us with some of equal interest in his *Therapeutice specialis*, (p. 250) and they are spread in sufficient amount through the writings of many of the older authors, to repay the

* Op: om: v. 3, p. 358—v. 6, p. 487.

trouble of research, to all, who think those ancient writers deserving of investigation.

It is apparently this "teazing" of the stomach and bowels, which (by sympathy) must call into action the ganglionic nerves. "Can it be credited," that *unless* the stomach and bowels were thus teazed, these organs would, nay could, sympathetically call into action the ganglionic nerves, and so impair the system, as to suspend its functions in a greater or less degree? Now, if this broad assertion of yours be correct, I would ask, whether this *impairing of the system*, must not necessarily be accompanied by some symptoms, indicating a change of organic functions, as preparatory (and happily therefore premonitory) to this "explosion of Cholera in its fullest force"? Are not the gastro-enteric affections of Broussais, in their highest state of explosion, essentially connected with all those little trifling sensations or premonitions, which, if duly attended to, would have precluded the accession of the higher grade of symptoms? Why should there be greater difficulty in considering the so called premonitory symptoms, a part of Cholera, than in looking to, and acknowledging as such, the early, mild and almost imperceptible symptoms of every disease, when compared with those more violent ones, which are to follow in succession, if not prevented by attention to the former?—All are in due order and rotation; and may be regarded as merely links of one common chain.

"Nor is this preclusive indisposition mentioned by any of the writers on Asiatic Cholera whom I have consulted. It is scarcely to be presumed, that so prominent and important a fact, had it an existence, could have possibly escaped the attention of these very able and experienced historians of the disease. Being attached to armies, and more particularly from their position in Hospitals, they enjoyed the best, and peculiar advantages, for accurate and discriminating observations."

Whom of the "writers on Asiatic Cholera," that do not mention it, have been then consulted?—I believe I do not mistake the intention of altogether denying, (in the preceding, the present and succeeding sentences,) all premonitory symptoms, or preliminary stage, here called "disturbances of the alimentary canal by nausea or purging, or the two united." If I do not misrepresent the meaning, which is remote from my intention, I cannot but think that its correctness is sufficiently disproved: nevertheless, to prevent all cavil, I will notice a few in particular, in order to shew, that if the fact of "this *preclusive* indisposition" has not been met with in any of the writers on Asiatic Cholera whom you have consulted, your reading must have been limited on one of the most interesting subjects of many years' standing! To say nothing of the numerous papers in the English periodicals of 1820, 1821 and 1822, and sub-

sequently up to the present moment, I need only mention the early Reports of Bombay, &c. in 1819, in most of which the fact of premonition is very generally adopted.—At a later period, when observation “and personal experience” had been more extended, and when those, who deem it right to confide in the reports of others, collected the reports in this particular; we find amongst others, in the “Practical Observations on Cholera Asphyxia,” by J. B. Kirk, that he not only accredited these premonitory symptoms himself, but obtained the testimony of numerous practitioners in favour of the same. To mention the whole would be unnecessary; I shall only state, that he has enumerated nearly 40, in which scarcely a difference of sentiment prevails;—one practitioner returns 78 cases of Cholera, *all* preceded by Diarrhœa;—another, gives 579, in which it obtained invariably, and adds, that by stopping it you save your patient—one of 600—of 500—and several of 100—of 50 and less. All unite in their opinion as to the premonition arising from Diarrhœa.—To come nearer home, the *New York Special Medical Committee*, in a paper dated August 1st, 1832, make use of the following language in relation to this subject.

“The Members of the Special Medical Council have been very careful to make the most extensive inquiries of the Physicians under their direction, and generally among their medical friends, concerning the premonitory symptoms of the disease; and thus far the important fact is confirmed, that the invasion of Cholera is, with very few exceptions, preceded by some notice of its approach—unless it had been brought on by a gross violation of the rules of living, as dictated by prudence and laid down by this council; intimation of its approach is most frequently given by uneasiness or looseness of the bowels. The results of our inquiries go likewise to prove, that in this stage of the disease it is very much under the control of medicine judiciously adapted to the particular circumstances of the case. A want of due attention to the premonitory symptoms, especially to a looseness in the bowels, is followed by aggravated Cholera, and too frequently by death. The certainty of great danger when this warning is neglected, and the equally strong assurance of safety when these symptoms have been removed by proper remedies, should induce every one to be watchful of the first appearance of diseases, and prompt in meeting them.”

To extend these authorities, seems almost useless; yet it may be well to advert to the state of opinion amongst the Medical men of Philadelphia, your cotemporaries;—perhaps it might be allowed to say of many of them, co-equals! The Commission to visit Canada, from Philadelphia, both in the short report made at their return in the newspapers, and in the more extended one of a later

period, insist upon it; especially in p. 22. Facts of a like import are largely disseminated in the pages of the Cholera Gazette of Philadelphia. Such, and so numerous are the facts on this head, that did we not know the diffidence with which some writers view their own opinions, it might be imagined that this difference arose from a desire of singularity! Even the ancients have in this, as in many other particulars of consequence, pointed out to us those facts, which we ought to bear in remembrance; *Cælius Aurelianus*, a writer well known, and of infinite merit, and who lived about two hundred years after the death of our Saviour, in his book entitled “*De Morbis acutis and chronicis*,” p. 255, lib. 3, cap. 20, expressly mentions the *Signa Cholerae futuræ*—“*Præcedit frequenter cholericos stomachi gravedo atque tensio: anxietas: jactatio: vigilia: tormentum intestinorum cum sonitu, quem Græci borborismon vocant. Ventris dolor: atque per podicem venti fluor nihil relevans*,” &c.—His full description of the disease is graphic as any of the present day; and for the benefit of those who do not possess his (oft-quoted but little perused) work, I might here give it, did time permit.—What makes me think he is actually describing the disease in an epidemic character, is the observation made by him, that it was so acute and rapid as described by the ancients, *as never to reach the second day*. Now the common Cholera, wherever it exists, though sometimes fatal at a very early period, can scarcely be asserted *never* to reach the second day. It may be worthy of notice, and perhaps may lead to a closer investigation, by those who vehemently cry up sympathy, as if before unknown, to look at the antiquity of the doctrine of the consent of parts, as applied to this disease. “*Magis autem patiuntur in ista passione stomachus et venter, et intestina: cætera vero membra omnia corporis consentiunt*.”—How nearly has he forestalled the views connected with the ganglionic nerves!—Not to break in, by further reference to him, I will merely remark, that he made great use of cups to allay the spasms, and for other purposes, to the limbs and epigastrium. They were not however accompanied with the scarifications and discharge of blood.—Let me now again repeat, that whoever has not met with all the authorities I have noticed, and many more on the subject of premonitory symptoms, must admit that their reading has been limited and imperfect; and consequently, inadequate to authorize so decided a denial, as is here done, by calling it merely “a condition,” and deriving it from “distinct sources of irritation.”

But I observe moreover, that in denying it to the writers that have been consulted on Asiatic Cholera, it is thought to be scarcely presumable, that so “prominent and important a fact,” (meaning premonitory symptoms) could have escaped them, if it had existence; and especially, since “being attached to armies” and “hospi-

als, they enjoyed the *best* and *peculiar* advantages for accurate and discriminating observations.”——I cannot doubt this to be your candid opinion; and consequently, as a just inference, that you would have much greater confidence in the observations made in the Locust street hospital, than in any derived from private practice.—But are we not all aware, that the fleeting and insidious symptoms, whether regarded as premonitory, or as a “condition,” were for the most part disregarded; and that it was rarely, until the disease was fully formed, and vomiting, &c. had made their appearance, that the patient was conveyed to the hospital? Was it not, in fact, a source of complaint in the public papers, that many, and especially in the beginning of the epidemic, were brought thither in a moribund state? And is it not generally admitted, that it is chiefly in the forming stage, and “not too far advanced,” that medical agency will prove beneficial?

“It was first noticed, and promulgated by some of the British publications, though not sanctioned by all, and from a similar coincidence of gastric and entretic derangement with the epidemic in this country, the notion has been espoused by us. Be it as it may, such disorders should at once be removed, as they are apt at all events, to invite an attack of Cholera. They do not differ from the ordinary complaints of the season, and require no peculiar management.

By most authorities deserving of consideration, on the subject of Epidemics, the ideas of precursory symptoms are no wise obscurely taught; and by attending to which symptoms, the full formed disease may often be prevented; and the old medical axiom be fully appreciated, of “*venienti occurrere morbo.*” It may, as is here stated, have been some British publications that first noticed the “hypothesis” in question; and if confined to *this individual epidemic*, there can be little doubt of the fact. Now the reason of this is conspicuous, in those publications being the fruits of English physicians, who first saw and wrote upon it, almost at the instant of its breaking out in India, in 1817; and the fact of *their* having noticed it thus early, is a strong presumption of its truth; whilst the *similarity* of those premonitory “gastric and entretic derangements,” wherever the Cholera has appeared, is another fact of equal corroboration. It will, I trust, be granted, that such “similar coincidence” in every part of the world, looks very much like the close connexion of cause and effect; in the opinion of most persons, it will probably be considered to be the truth, and not a mere “notion,” as it is here denominated. Nor would it be difficult to shew from your own writings, whether on your own authority, or, as pointed out by Webster in his work on Pestilence, (a book of *infinite convenience* for reference, and proof of the energy and laborious investiga-

tion of its author among the old writers) that an opinion of this kind was common in former periods, and has been deemed orthodox by you.—May I not indeed affirm, that the assumed connexion of the prior symptoms (ordinary complaints of the season) with the Cholera, is not imaginary, but truly founded, since you admit that “they are apt to invite at all events an attack.” Now this is all that is contended for; and is that which is to be sedulously guarded against;—if successful in arresting the further progress of this “condition,” Cholera is effectually prevented. Nevertheless, if this “coincidence” is common, and if apt to invite an attack, must we not think the term premonitory, at least judicious, as a beacon to light up the approach of the enemy; and thus aid in frustrating his attack? It leads to nothing injurious, either theoretically or practically, and I would therefore suggest a re-consideration of this “notion.”

“Genuine Cholera, for the most part, comes on with little or no premonition. The earliest symptoms are complaints of load, and oppression, and anxiety about the præcordia, with an internal sense of heat, referrible to the stomach or bowels, with great thirst and a whitish tongue, and at the same time the head is confused, the expression of countenance haggard, accompanied by slight nervous tremors, muscular weakness, cold skin, and either a quick and somewhat feeble, or a full and struggling pulse. Copious evacuations upwards and downwards, of fluid resembling dirty or turbid rice water, with flocculi mixed in it, soon occur, followed by cramps or spasms of the muscles of the extremities and abdomen. These are seldom so violent as has been represented, and never extend to the alimentary canal.”

By “genuine Cholera,” in the sentence, I presume is meant the violent, spasmodic, asphyxial, Indian, or pestilential disease; which is thus indirectly admitted occasionally to come on with premonition. Whichever of these names is adopted, the term may be regarded as incorrect, inasmuch as it is not *always* accompanied with spasms, asphyxia, or pestilential symptoms; nay even the term Cholera is improper; since so far from bile being generally discharged, it is, both upwards and downwards a fluid altogether different; and sometimes, as is mentioned, (confirming thereby the long line of authorities from Hippocrates downwards,) there is an absolute absence of vomiting or purging. What shall we call this variety? a cholera sicca, as the older writers have done? for we cannot well do without some kind of name. As however, none of the symptoms are uniformly present, we cannot well tell on which to fix as pathognomonic of the complaint, and by which our nomenclature should be framed. “The *earliest* symptoms of load,” &c. are in truth, those very premonitory symptoms which you decri; but which every prudent person, medical or otherwise, will instantly attack, before the higher range of symptoms follow; and in so doing, collapse, congestion, &c. are all annulled. They are the very symptoms which

you tell us “do not differ from the ordinary complaints of the season,” and which “should at once be removed,” because “they are apt to invite an attack of Cholera.—Had you attended sufficiently in drawing a line of demarcation, it would have probably been found, that even these “earliest symptoms” came on progressively, or consecutively; the earliest being the first link of the chain; and therefore necessarily the earliest to be arrested by remedial measures; whilst the later named symptoms of the catalogue, indicate the approximation to, or the full formed disease. All, it may be affirmed, is in strict order; and is reasonably deducible from the harmony of nature, even in her state of morbid action upon the animal economy.

The expression of “*either* a quick and somewhat *feeble*, or a full and *struggling* pulse” amongst the earliest symptoms, would appear to indicate much diversity of disease; or, that this part of the picture is really drawn from different periods of this early stage itself; at any rate, they would seem to prove, that much difference of treatment may be called for, to meet their respective indications. Is not “the somewhat feeble pulse” rather in opposition to “very free bleeding”? Perhaps this “somewhat feeble,” falls within the limits hereafter adverted to of “false appearances of debility,” by which you say you were “deluded;”—and probably, when the delusion ceased, *that* apparent state of debility resolved itself into one of oppression or congestion; relievable by the evacuant and depletion plan, rather than by the “course of practice” you at first adopted.—For it is well known, that congestion is more readily removed by depletion, than by super-addition to the causes producing it: hence the high importance of blood-letting, which would seem to be claimed as your discovery, in the postscript to your amended copy; but which I shall endeavour to shew, in its proper place, that even if this practice has been taught “for twenty years in your lectures,” yet, that having abandoned those principles, an exclusive claim upon them, could not be correctly sustained.—Let me here ask, what is meant exactly by a *struggling* pulse? I have looked in vain, into several writers who have nearly exhausted the subject, but find none under this descriptive title. Whatever may have been meant by *struggling*, I suppose the “*full*” means a *strong* pulse, in contradistinction to the quick and feeble one. All this will doubtless be explained hereafter.

I have already adverted to the occasional absence of the “copious evacuations,” &c. chiefly with a view to attaining a name for the disease, that shall be free of ambiguity or contradiction! The “*Lucus, a non lucendo*” might be a fair premonitory plea, for Cholera, *sine ڪولہ*. Not only they are occasionally absent, one, or both—but they never or rarely are coeval with the preliminary stage. It is said in the sentence under notice, that the spasms *never* extend to the alimentary canal. Perhaps it is *rarely* the case; but surely,

an attendance on a few patients, can scarcely authorize a decision so unqualified ! If I am not mistaken, others have found evidence of it, in the *irregular contraction* of the intestines, continuing even after death. These spasms of the extremities, &c. are very curious, and will no doubt receive attention. May we be allowed to ask your opinion as to those highly singular cases that are noticed, in which the muscles continued to act, a long time after death ? Is this sympathy, or what ? Perhaps, as you do not mention them, you did not meet with any such cases ; some therefore may possibly deem it incredible : I think it proper to give one case, at least, from Mr. Finlayson, in the *Medico-Chirurgical Transactions* for 1820. One would almost suppose that Galvani had located himself within the individual, and was performing some of his electro-chemical experiments.*

“An aggravation of the preceding symptoms rapidly takes place, and in half an hour or more, the tongue becomes icy cold, the skin more chilled and sodden, tho’ feeling hot to the patient, covered with a dewy viscid perspiration, the hands shrivelled or wilted, as if mascerated, the nails of the fingers blue, the pulse scarcely or not at all perceptible, the face sunken, especially the eyes, around which is a dark circle. This colour gradually diffuses itself over the entire surface, partaking of the various shades of lividness, from a saturnine to a blueish or blackish hue.”

As no two authors probably, in the description of this disease, give it uniformly in the same manner ; and, as infinite diversity must be produced by the different constitutions on which the æriform poison acts ; so we may, perhaps, consider the statement and train of symptoms here given to be as good as any. I must however request attention to one or two particulars, in which I apprehend, you have employed rather a *figure of speech*, than given us the actual fact. It is well known, the temperature of the body is about 98° ; and, that during the month of August, the thermometer rarely sinks below (say) 70°.—Now, *icy-cold*, as applied to the tongue, would im-

* “The other died 14 hours after seizure, the most powerful remedies having been employed without effect. About 15 minutes after he expired, the toes began to move in various directions, and the feet were made to approach each other. Muscular contractions were speedily propagated upwards, along the inside of the legs and thighs. The thighs were turned slowly inwards, so as to approach each other, and again outwards, the whole of the lower extremities moving on the heels as on pivots. These motions proceeded upwards, producing a quivering in the muscles. In 5 minutes the upper extremities began to be similarly affected, the fingers were extended, and often rigidly bent inwards ; pronation and supination of the hand were steadily though slowly performed. The same quiverings were observable as in the lower extremities, and extended to the pectoralis major muscles, and the superior margin of the latissimus dorsi. The muscles of the face moved, and the head was observed to shake. The total duration of these appearances was half an hour. By moving or pricking the arms or limbs, these contractions were rendered stronger, and again renewed when they had ceased.”

ply 32° at least. Did you ever try the thermometer to determine the fact? or did you, without duly attending thereto, feel it, just at the time, or immediately after the patient had taken into his mouth a piece of ice for which he had an "instinctive" desire? I am, I confess, doubtful of the tongue having ever fallen in temperature to the freezing point; for I have nowhere found that it becomes quite rigid and frozen from this diminished temperature; but which would be the case very certainly, in a short time, if the fact was as your words imply.*—The chilled sensation of the skin, (to the physician we presume) whilst feeling hot to the patient, is one of the most singular facts of false perception that we find in the annals of medicine, and extremely difficult to explain by any of the hypotheses that have exploded on the subject.—What renders it more curious, is, that it is nearly the reverse of what we find to be the case, in the *cold* or *congestive* state of Intermittent; and in which bleeding has also been so strongly commended, though not generally adopted!—It is probable analogies will be found between them; especially if we refer to the intermittent Cholera of Hoffman, Torti, and others already alluded to. In this state of chilliness, whilst the patient is shaking universally—his teeth chattering, and the sense of warmth is scarcely appreciable by him, though covered with blankets; yet to the attendants, the temperature of the body is greater by several degrees than the natural.†

In saying the *nails are blue*, we perceive merely an echo of the

* It is the more to be regretted, that, (so far as I have heard,) the thermometer was not tried in the above cases; from the circumstance that Mr. Keir in his treatise on Cholera, as it appeared at Moscow, speaking of the diminished temperature of the tongue, p. 57 and 58, adverts to two cases, in which he measured the temperature under the tongue, by the bulb of Reaumur's thermometer placed there for two minutes. In one of them it was at 20° or 77° Fah.—In the other, at 25° or 88° of Fah.—Whilst under his own tongue it reached to 30° or 95° of Fahrenheit. These cases were amongst the worst, and yet no *icy-coldness* seems to have been present.—We ought to have had a comparative experiment here, especially, since Keir's treatise, or at least Boisseau's, who refers to this experiment, were in the city of Philadelphia at that juncture.

† Something of an analogous character, and therefore deserving of association in this place, is to be found in an account of an operation for Aneurism in the ham, by Mr. Brodie, at St. George's Hospital in 1827. The operation performed in the usual manner on the 9th of August, seemed to progress favorably until the 6th of September, when a 2dary hemorrhage appeared, and augmented from time to time, so that on the 8th, Mr. B. tied up the artery in the groin. On the 11th Oct. hemorrhage ensued, and was restrained by pressure, but "at 4 A. M. of the 19th, 12 oz. of blood were lost from the groin, and on the morning of the 20th, a pint and a half"—"From the 20th to the 50th, the patient went on very well. No hemorrhage occurred; the pain in the foot was relieved, and he was recovering from the state of depression into which he had been thrown. On the morning of the 30th, however, a slight bleeding occurred, with occasional rigors; and, *what is curious, although he was shivering, and complained of excessive cold, the temperature of the skin was up to 103° by the Thermometer.*" This man died on the 2nd Novem.; and the whole case is extremely interesting, not only in itself, but from the subsequent severe critique of the *Lancet*; in which it appears, in the No. for Feb. 23. 1827-8, p. 791, &c.

mistake of every writer. The semi-transparency of the nails, as horn, permits the blood congested below them, of a dark or livid hue, to be seen:—but the nail, as such, is certainly untinged. It is perhaps of little importance, further than that it is well to understand as far as possible, the real character of every symptom; and hence, we find in fact, a lividity *around* the nails, some time before it is distinctly marked upon the nail itself.—The affirmation here given, of the pulse becoming “scarcely or not at all perceptible” in “half an hour,” is properly qualified, by the super-addition of the words “*or more*”:—meaning thereby, we presume, an indefinite and unlimited period, which any one may fill up, *pro re nata*; and therefore, as well calculated to apply to those cases, in which the accession “was as sudden as the electric shock,” as for those of a more protracted character.—This colour, we are told, *gradually diffused* itself over the *entire* surface.” It may be doubted if this expression is quite correct: wherever the remora of the blood exists on the “periphery,” there it will be seen; and exactly in intensity to the degree of that remora, or congestive state.

“During this period, the thirst is intense, the heat of the stomach in some instances is increased to a burning sensation, the respiration greatly embarrassed, the air expired cold, the voice low, or whispering and plaintive, the diaphragm convulsed, and there is a total suppression of the urinary and other secretions. Discharges from the alimentary canal, and the spasms, likewise cease or are much diminished. Death ultimately takes place in a sort of tranquil stupor, or with indescribable agitation and distress, the latter state being by far the most common.”

The intense thirst which is here spoken of, appears to be one of the most prominent, and permanent symptoms; and for the most part *comes on, earlier* than this sentence would seem to imply; continuing with almost unabated importunity to the very instant of death. This is doubtless, in some way sympathetically associated with the burning sensation of the stomach; but more so, we apprehend, with the exhausted state of the blood vessels, and consequent diminished secretion of the saliva and other arterial discharges. The instinctive desire for fluids, to supply the watery discharges *κατα et ανω*, points loudly to the proud practitioner, that, after all, he is but the handmaid of Nature; and happy will he feel, if not too desirous of obstructing the measures she not obscurely points to!

We are here not favoured with the slightest explanation of the “embarrassed” state of respiration.—Do you suppose Sir, that it *always* depends on one and the same cause, through all the various stadia of the disease? Too much stress is probably laid by writers on congestion alone; since *many* dissections seem to shew, that the lungs *are not always*, perhaps not commonly, in such a state.—In the cold stage of Intermittent, attended with congestion, the air.

returned in expiration, *is not cold*, as is found to be the case in Cholera;—not even in engorgement, or apoplexy of the lungs.*—Now, is it not plain, that when the embarrassment, in respiration is accompanied with cold breath, that such embarrassment must depend on *something else* than mere congestion, at least of the lungs? But what is that? May we not say with some degree of certainty, that, since the pulse is absolutely stopped, and the heart, apparently has ceased to propel the blood, (for the larger portion of what is left is collected in the venous system), which no longer finds a passage through the lungs, and therefore that organ has little, if any to propel; may we not say, that if any *apparent* respiration remains, it is but apparent, and useless in the existing state of things? What can respiration be wanted for, if no blood is passing from the right to the left side of the heart? The object of respiration, is decarbonization of the blood, and the promotion, in some manner, of animal heat.—If then, no blood passes by the pulmonary vessels, the lungs will not be in a state of congestion—and yet neither decarbonization nor animal temperature are effectuated; the patient is just as devoid, in fact, of those functions, as in asphyxia from drowning or hanging. Hence the air inspired, if so it can be called, returns with little or no change of temperature or property; a minute portion of carbonic acid only is evolved, and the temperature of the body necessarily must sink down to at least that of the common atmospheric mass. It is obvious therefore, that respiration, such as it continues to shew itself, is but a mere *singultoid* (if I may coin an expression,) motion of the diaphragm, unaccompanied by any regular muscular action of the intercostals!—Is this what is meant, when you speak of the diaphragm being convulsed? and is this explanation in any wise allied to any hypothesis you may have formed thereon?—A *duo* consideration of these co-operating causes, may possibly, also serve to illustrate and explain the “low, whispering and plaintive” voice.

As for the *total* suppression of *all* the secretions, perhaps we may except the sweat, which I believe is regarded by many as such;† for, although necessarily cold, from the absence of the function by which animal heat is produced, it is nevertheless present, even to the last. Now this is very extraordinary, when we take into consideration, how greatly the blood becomes inspissated, so as often to resemble tar or molasses. Yet, by venæsection, it has been found to regain its former florid hue, and just consistence. Somewhat

* It occurs in some cases of immense exhaustion by Uterine hemorrhage, as is noticed by Dr. Blundell, in his lectures on Midwifery.—Vide *Lancet* 1827–8, p. 580, and elsewhere. May it not be imagined, that this symptom, arising from extreme exhaustion in Uterine hemorrhage, by which little if any circulating fluid passes the lungs, is explicable in Cholera from a similar cause?

† And probably the hepatic secretion (a venous one), for the gall-bladder was rarely found empty, nay for the most part greatly distended.

analogous to this, will undoubtedly be recollected to be developed by Dr. Rush, who in his treatise on the Yellow Fever, says, (p. 146) "I paid no regard to the *dissolved state of the blood*, when it appeared on the *first or second day* of the disease; but repeated the bleedings afterwards in every case, where the pulse continued to indicate it. It was common to see sizzly blood succeed that which was dissolved."—In the amended copy of your letter, you have here introduced the fact, that "the intellectual faculties, though obtuse, are seldom otherwise affected, and in some instances, their entire integrity is throughout preserved"—but we are not favoured with an explanation of this singular, but interesting fact.

"As I have briefly described the disease, such is the tenor of its character and progress; though occasionally diversified in some respects. Thus I have seen its accession as sudden as the electric shock, and have met with cases without spasms, or vomiting, or purging. Many other anomalies might be mentioned, could I indulge in such details."

No doubt, great varieties must have necessarily occurred in the character and progress of this disease. Every writer upon the subject for fifteen years past, has recorded them in a greater or less degree; and you must feel that you were extremely fortunate Sir, in your limited practice of three weeks, to witness such a vast variety, independently of the other anomalies you have omitted to indulge in. It is to be presumed, that all this is derived from your own practice alone; inasmuch as you limit your views, in your letter, to your own experience and observation.—In the cases adverted to, "sudden as the electric shock," there certainly could have been no time for premonitory symptoms; that is, provided it is conceded that this "sudden" attack, was *absolutely coeval* with the reception of the "æriiform poison" into the stomach; and that the sympathetic influence thence radiating upon the ganglionic nerves was equally rapid! Those, who are less tenacious of the undefined powers of sympathy, and who advert to the admission, that you "know nothing with certainty" of the epidemic agency of this disease; may possibly await *their own* actual observation and experience, to satisfy their minds, whether, rapid as the accession of the disease appeared to be, there is not a strong presumption, that the poison had been slowly acting on the system for a long period, and, like fuel in a fire-place, required only the fortuitous concurrence of a spark to set it in a blaze. As fuel without the spark will not ignite; and, as the spark itself, will be but momentary, without the conjunction of the fuel; so it may be admitted as probable, that *every system* in Philadelphia, was charged with an unknown æriiform poison; and yet, those alone be attacked, who inconsiderately applied the match, by some inat-

tention or excess, &c. ! Diversity of constitution, would co-operate with other causes, to induce a great diversity in the character and progress of the disease; and thus, an agent, which, if efficient *per se*, must also be uniform in its action ; *appears* to vary in that action, merely through the variety of the recipients it meets with. We may properly therefore, object to the term of *anomalies*, as calculated to convey a wrong idea of Nature, even in her morbid actions; when the accusation should be made against our own ignorance, or imperfect observation of her laws in the animal economy !

“The disease may be properly divided in most instances, into two stages,—that of aggression, and collapse.”

No particular objection presents itself to this division into the two stages of aggression and collapse, further, than that, as it is altogether artificial, it is not prudent to speak of it as if it was absolutely proper. Every reader is no doubt aware, that others have made more numerous subdivisions, and possibly, on the whole, superior to this;—although, as above remarked, all are artificial ! You obviate, it is true, some apparent objections, by the term “in most instances,” adding hereby, another proof of its proteiform character.—The symptoms of the assumed stage of aggression, (at least several of them), appear strongly characteristic of those *premonitory* ones, so generally admitted by the majority of writers on the subject; and it is therefore to be regretted, that we have not the exact line of demarcation drawn, between the two stages into which you divide the disease.

“Called at the commencement of an attack, unless there is extreme depression, I bleed very freely from the arm, and *uniformly cup the epigastrium*, and give calomel largely, combined or not with opium, according to the severity of the spasms. The case will almost invariably yield to these remedies, and we have no further trouble concerning it.

Pray Sir, inform us more particularly, of what is here called the “commencement,” and how it is to be recognized. In other words, how we may exactly discriminate between those “mild and lingering affections,” that for three or four days have been “teazing the stomach and bowels,” and the symptoms of the supervening disease ? As you have given an absolute denial of their connexion, it behoves you to point out clearly, to those who have not your tact and powers of discrimination, the precise and imperceptible line, which terminates the one and originates the other ! To Dr. Tyler, (anticipating doubtless the disease,) such information would have been

invaluable, as well as to all those who looked forward to the disease amongst them; and, as yet, had no self-experience in it. To the Philadelphians, it was of less importance, because your letter did not reach that city until the disease was on its departure! The word "*unless*," put me greatly in mind of the lawyer's *if*, in the fable, as it seems well adapted to apply to any thing we may think proper, either at the present moment, or at a subsequent period; so that it will answer an admirable purpose, as a medical *nisi prius*!—When you speak Sir, of "extreme depression"—unless you mean debility thereby, I should suppose your "very free bleeding" would most promptly and readily remove it; and I therefore the more regret that we are not afforded the distinction between it and those "false appearances of debility" to which you subsequently allude. At the very commencement of an attack, it seems scarcely possible to imagine the prostration of the system from a state of perfect health, to be so immediate and extreme, as to prohibit bleeding! May not an *oppressed* state of the system have therefore been mistaken here, for one of debility; and thus occasionally led to a wrong indication? This *apparently* debilitated state, (nay the absence of pulse even), has not precluded many practitioners in India, Europe, and elsewhere, from copious depletion, as the readiest mode of relief. They did not regard this "extreme depression" as always analogous to debility. The Madras and Bombay reports, are replete with the recommendations of copious bleeding. The resemblance of this disease to *congestive* fever, was even pointed out in 1819, although lately assumed, as if recently imagined! The venous congestion was found to constitute the principal change, as evinced by dissection. In almost every case, the first symptoms were of this character; so that even then, the disease was divided into two stages, viz. that of oppression, and that of collapse. Bleeding, calomel in large doses, and opium, are all strongly commended, together with bathing, frictions, &c.—And it must highly gratify those early writers, to find, that (after fifteen years' continuance) the practice recommended by them so long ago in India, has been laid down in your letter of 1832!—If more is desired on this point, we can direct attention to the cases by Dr. Walker, in the Edinburgh Medical and Surgical Journal for July, 1820. Here we shall likewise find profuse bleeding, &c. equally commended; and in almost all the periodicals for 1819, '20 & '21 and consecutively, may be found numerous writers urging the same depletory plan of treatment from the very "commencement." Mr. Kinnis, in the same Journal for 1821, at the Mauritius, has given an excellent view of the disease, from its commencement to its termination, in which every symptom here enumerated, finds a place. The average length of the disease was 9 or 10 hours; bleeding and calomel are among his primary measures.—And Lloyd, in the same Journal, mentions the ab-

straction of 45 ounces of blood, apparently at once, and followed up by calomel and jalap. These references may suffice to shew the *early* disposition, from *actual observation and self-experience*, to pursue “with scarcely an exception, the depletory and evacuant” plan of treatment;—for they seem not to have been amongst the number of those practitioners who, we are informed, were so “prone to abandon their principles, &c.—And hence, they appear to have been commensurately successful; since we are told (Johnson’s Med. Chir. Rev. 1832, p. 628) that the mortality in India was not *one in seven*; whilst in Europe it was nearly one in two—in England as one to three nearly.—What it has been in Philadelphia, the accompanying table will partly evince; and what, in each individual hospital, may also be seen.—I observe Sir, you “uniformly” cup the epigastrium! I presume therefore, it was a standing rule—requiring no distinction of patients, but employed on all, as a matter of course!—I wish you had stated precisely what the words “almost invariably” imply; I mean the actual number of cases that have been found to yield to the remedies you recommend, as it would have enabled us to compare the success of the respective hospitals.

“But where *the attack is confirmed*, or in other words, the state of collapse exists, the difficulties of management are vastly increased, and the practice is somewhat different. The first step under such circumstances, is to puke actively with tepid salt and water, a tumbler full at a time. This usually settles the stomach, allays thirst, produces some degree of re-action, a stronger pulse, increased warmth of surface, and a resolution of the spasms.”

“But”! Here is another word of various import! We have unquestionably in this sentence, a reasonable truism, both in regard to increased difficulty of management, and the difference of practice! In other words, it is presumable, that, the forming, preliminary or premonitory stage having been suffered to pass, and the attack confirmed; diseased actions, and vitiated secretions, proportionably augment; whilst the system is relatively less able to support them. It cannot therefore be difficult to comprehend, that a practice expedient under one set of symptoms, must be greatly modified to meet the indications presented by another; unless, indeed, we become dismayed by the fearful character of a disease, and abandon principles and well tried remedies, in analogous cases, as you inform us, some “practitioners” did: and who, in the estimation of the medical republic, will I trust, although practitioners, not be regarded as authority in Medicine! Who they are we know not; but, if there are any who from dismay, did *actually abandon their principles*, for one I shall venture to declare, they deserve to be abandoned by the honorable profession they have thus disgraced!

Let us now, notice wherein this difference of practice consists.—“The first step,” you tell us, under the circumstances “of collapse,” is to puke actively with salt and water. You do not claim this of course; I mean, either the *puking* actively, or the salt and water; for we know that before you employed the practice, it had as many opponents as advocates! What led to your employment of it; and when; so immediately after your first acknowledged failures, does not appear. The article is mentioned as the “Russian practice,” in the “remarks,” by your assistant—who, (as he quotes Johnson’s Med. Chir. Review for April last,) must have necessarily read much on the utility or inutility of the various modes of practice therein pointed out. The importance of *venæsection* in overcoming “the universal venous congestion,” is such as to have “no substitute,” and * we are moreover expressly told, “the blood must be drawn *from the veins*.” This important fact, I presume, although not noticed, is duly ascribed to its proper authority; for it is to be found in Johnson—and could not be intended to be claimed by your assistant, although it might be so considered by his readers; particularly as it is spoken of conjointly with other measures, as among the “*grand principles of our practice*,” founded upon the pathological views of this disease, now offered to the medical profession in these remarks.” At all events, difficult as it is acknowledged, *venæsection* often is, in this disease, Arteriotomy would probably be as much or more so, from the arteries being nearly empty, as compared to the veins.

To recur to the salt; we are told to “puke actively with tepid salt and water, a tumbler full at a time.” Now, although salt and water may be a safe remedy, we must have some regard to amount. Yet you do not state the definite quantity of salt and water—and appear to have forgotten that we have tumblers that will apply both to Lilliput and Brobdignag! Which should we then employ? and how many usually will settle the stomach?—Should the circumstances happily follow as here indicated, the prospect is certainly cheering; and I hope a regular journal has been preserved, both of the practice and the amount of its success, until the date of the letter.

“Co-operating in the same design of arousing the vital forces, and exciting the skin particularly, rub the body and extremities with warm flannels. Let a vein be then opened, and if the blood flows freely, take a large quantity, and especially should the pulse rise and the blood become florid. But where the reverse happens, or you have slowly to coax out the blood, or the pulse is sensibly weakened by the loss of it, stop the operation, and apply twenty or thirty cups to the abdomen, including the epigastrium, which, though they may not draw much blood, are eminently serviceable as revellents. The cups are to be succeeded by a blister to the same parts.”

* *Am. Jour. of Med. Sci.* August, 1832, p. 539.

This design of arousing the vital powers, and exciting the skin by rubbing with flannels, is excellent to promote metastasis, &c. It is, we know, the good old women's plan in such cases as require counter-irritation; and they not uncommonly, are so bold as to add to the flannel, a little camphorated spirits, ether, brandy, hartshorn, mustard, and so forth, to excite still more the body and extremities.

In relation to the recommendation of opening a vein, and taking a "large quantity" of blood, *if* it flows freely; this is admirably guarded by the little word *if*; for it is well known that in this disease, blood will *not always* follow where the *lancet* stabs. We have already adverted to the tenacity, spissitude, or diminished liquidity of the blood, so that it often will not flow, even by the coaxing recommended. Now, time is running on, if the blood will not; and if this should not run *freely*, are we to infer absolutely, that we *must not*, or that we *cannot* take away the large quantity mentioned?—But, as a *large* bleeding is merely relative, I could have wished therefore, to have had some precise ideas of it, as applied to *this stage* of the disease, in contradistinction to the previous stage "of aggression," in which you also bleed "very freely;"—and consequently, there seems no very great diversity of practice in the two stages.—It will be seen here, what additional reason there is for regret, that you have not established the difference between bleeding *very freely* in the first, and a *large quantity* in the second stage. Such vague directions may do for one of the populace; but it will be admitted, that the vox populi is not always, nor often, the vox Dei, either in relation to Divinity, Law, or Physic! especially the latter, when the utmost precision is required; and when, the difference of a grain of some articles of the *Materia Medica*, might prove the difference of time from eternity.

The blood becoming "florid" by bleeding, and the pulse rising, (indications unquestionably opposed to debility,) doubtless led to the connexion of the previously mistaken views of the disease, with an opposite state of the system; or rather, that a state of congestion, and not of debility, was really present. In every disease apparently, in which respiration is "embarrassed," so as to preclude or diminish the decarbonization of the blood, this fluid necessarily assumes a darker hue: but it has possibly never before been noticed, that this alteration of colour, occasionally occurs, even in the arterial blood.—This disease will probably greatly modify many of our physiological and pathological notions—and bring us back to a proper association of the at-present disunited doctrines of Solidism and Humoralism!

You proceed to state, that "where the reverse happens," &c.—By this, we are led to suppose, that it could not with certainty be determined beforehand, whether the blood would flow freely, so as to extract a large quantity, or whether it would have to be coaxed

out.—But why stop it? why not continue the operation of coaxing, if bleeding was necessary? Why is not explained the mode of coaxing, of which possibly, Dr. Tyler and other readers might be ignorant?—If the coaxing is continued long enough, will not the amount of blood evacuated, equal expectation; and perhaps by this slow process, be even better on some occasions than a more rapid flow; by allowing the gradual resumption of the diminished or oppressed action of the vessels, as evinced by the gradual rise of the pulse—especially too, since this slow tapping of the veins, need not preclude the 20 or 30 cups spoken of, “to the abdomen, including the epigastrium” (I always before this, thought the term abdomen *did include* the epigastrium, as the greater includes the less *).—This last named part, being uniformly cupped in the commencement, must now be in a fit state to receive this additional irritation, and prove, that, *ubi irritatio, ibi fluxus*. Here is probably, an exemplification of metastasis of engorged fluids from the mesenteric and other deep vessels to the periphery; for unless in this way, we cannot well explain what expectation is anticipated from them! We may here remark, that the super-position of the “blister to the same parts,” provided the patient lives 5 or 6 hours longer, in this confirmed state of collapse, must be irresistible.—May I ask, under all the circumstances detailed, if the *dry cupping* of Cælius Aurelianus before adverted to (p. 18), might not be equally advantageous, as mere “revellents”? And, whether the blister is applied as an evacuant or a stimulant?

“Calomel is next to be given in the dose of five, ten or twenty grains, frequently repeated, till the aggregate amounts to about a drachm, and then worked off with a table-spoonful of castor oil. As the results of these means, there are commonly bilious evacuations, discharges of urine, and other proofs of the restoration of secretory power.”

Is it meant Sir, by the word “*next*,” that we are to wait until all the above means had been put in operation, (bleeding, cupping, blistering, &c.) before we are to administer the calomel? Such, I apprehend will be the impression of most readers.—Consider Sir, the disease kills in from 3 to 12 hours! Time flies, and medicine is tardy in its effects, especially in a body, where most of the functions are suspended or diminished; and we know that “*Medicina non agit in cadaver*”! We have, all things considered, but little time to lose; nor can we say with certainty, either as to cups or bleeding, &c. that it is “cut and come again.” Rather may we say, that the motto, of both the physician and the disease, is now, “*vincere vel mori*.”

* “*Abdomen, est venter infimus,*” &c.—Blancard.

“*Epigastrium, est anterior pars infimi ventris*”—*Ibid.*

The calomel mentioned must be *next* given in doses of 5, 10 or 20 grains, frequently.—This seems in the case before us, one of those vague directions, that it will be difficult to follow: for, if given to the amount of “about a drachm” at an hour’s interval, in the respective doses mentioned, (and which in common diseases would be called frequently,) we shall give it in 12, 6 or 3 hours, according to the dose: a difference of some importance; and scarcely less so, if at half-hour distance only.—If the disease could certainly be depended upon, for such continuance, or longer; to enable the super-added castor oil to work off the calomel, much, no doubt might be anticipated from it: but as this is not the case, we are puzzled to understand what is meant by “frequently” in this stage or state of the disease.—As to the discharge of urine, so long suspended, and now renewed, and which is ascribed to the *restoration* of the “secretory power,” I may ask, what evidence do we possess of the *loss of* any secretory power in the kidneys? There is but little arterial blood carried to them indeed; but do dissections prove them to be much disordered? or are they so peculiarly under the influence of the ganglionic nerves, as to feel the impression of the æriform poison in a superior degree to almost every other part? Will not the vast evacuations of a saline and serous fluid by another channel, sufficiently elucidate its diminished discharge; especially as the arterial blood is nearly non-existent? This discharge being (*qua data porta*) by the intestines, must, we should imagine, be an adequate source of diversion from those organs.

“Little more is demanded than what has been mentioned. I have, however, sometimes known, though rarely, that at this point of the case, irritability of the stomach to return, with the appearance of approaching exhaustion, in which event, stimuli are to be resorted to; the best of which are, a strong infusion of cayenne pepper, *or* clove tea, *or* the spirits of camphor, *or* the aromatic spirits of ammonia, *or* mint julep. But they are cautiously to be administered, and in small portions, *or* they are instantly rejected, *or* they overwhelm the energies of life, *or* more slowly induce typhoid prostration.”

It must be admitted, that but “little more” can be demanded! If these measures are insufficient, what more can we reasonably employ? It is probable that in a very short period, death will claim his victim; for it is really now, a struggle between nature and death; in which the doctor has little else to do, except to look on, and reflect how far he may have contributed, either by actual prescription, or positive omissions, to the existing state of things! and how far this “return of irritability of the stomach” can be ascribed to the one or the other.—It is true, this occurrence is mentioned as *rare*! It is to be regretted, that the number is not more distinctly noted. However, we may perhaps approximate it, from the hospital returns,

up to the date of the letter; and which by the table annexed, will be perceived to have been 37 cases, and 16 deaths. Assuming all the recoveries to be real cases of Cholera, viz. 21; and as no doubt, a number of these came in at the "commencement of their attack,"—and, as such cases, under the treatment recommended in this stage of aggression will "almost invariably yield"—we shall perhaps be safe in counting them as half the number. The residue, (say 11) are those to which, in any way, the indefinite expression ("*sometimes*") in the sentence will apply. Now, this *sometimes*, qualified by "*rarely*," will at least, reduce this number to 5, or about one-half—on which your observations have been conducted, and your experience attained, as to the important application of stimuli, in opposition to the depletory and evacuant plan! By this time however, we may well imagine, that the "exhaustion" is real, and the debility no longer delusive.—Six, eight, ten or more hours, of this mortal strife, must have reduced the patient to the last extremity; and nothing remains to close the door against death, but to keep up a perpetual discharge of infusion of cayenne pepper, *or* clove tea, *or* spirits of camphor, *or* aromatic spirits of ammonia, *or* mint julep!—The cayenne pepper has long been known and used in the West Indies as a powerful, prompt and pure stimulant, and no doubt is useful, as well as the ammonia, in such cases, *if not too far advanced!* Of the doses in which Dr. Tyler might employ all these, we have no information: this must have been a great desideratum, unguided as he then was by the light of his own experience. It is true, the papers were at that time ringing the praises of three drops! of Tr. of camphor!—and it might not therefore be necessary to remind him of what he could elsewhere find.—Every one also knows that mint julep is an excellent anti-fogmatic, and as such, no doubt, had long been employed by that very class of patients who most frequented the hospitals. The propriety of the measure, independently of the numerous cases benefited thereby, must be apparent, from the influence of sympathy in renewing those associated motions, which had been dissevered, by the ravages of the disease.—I have said above, that the doses of these stimuli are not stated.—It is stated, it is true, that "they are to be cautiously administered, and in small portions." Now, it is from the great danger which threatens, and which is so ably depicted in the rising chain of events, and so happily linked by the little but expressive word *or*, that we have to regret, that the absolute amount of each dose of these agents has not been mentioned;—how many cases were noticed, in which this instant rejection, or overwhelmed energies, or typhoid action, followed their "ine cautious" administration? It will be perceived, that I ground all my remarks, on the supposition that what is stated, is the result of your own "actual observation and experience"—disclaiming as you do, that of others.—Here

when we see the many chances of failure of the above stimuli, if not cautiously administered, and in small portions; and yet we have no determinate directions given as to these important points. Apart however from the "small portions," allow me to ask, wherein this cautious administration absolutely consists, by which such an assemblage of evils is to be prevented?—for it needs the hand of a master to guide our footsteps in a path so perplexing!

"Drink is sometimes vehemently solicited, particularly in the height of the attack, and the *instinctive* desire for cold water, or even for ice, may be gratified in moderation. The proper nourishment in convalescence, is chicken water or beef tea, rendered agreeably pungent with cayenne pepper."

If this desire for cold water or ice is *instinctive* as is affirmed, why restrain it? To be sure, an assistant mentions a reason, which, if sufficient for such restraint, must rather operate unfavourably to instinct; and I should be glad therefore to have it reconciled. He says, (Am. Jour. of Med. Sci. August, p. 535) "I have known the *water and melted ice* (can you tell the difference of these?) that have been allowed to a patient at intervals during the space of an hour to be thrown up from his stomach *nearly* as cold as they went down."* It might have been well if he had examined this "nearly" by the thermometer. The fact is however sufficiently established—though the reasoning therefrom may not be conclusive: but may we not, in admitting this asserted *instinctive* desire, suppose, that it was from not permitting its full sway,† that many cases may have proved fatal? How do we know in this stage or state of the disease, what constitutes moderation in drink?—At least one case may be pointed out, that was cured apparently, by the use of cold water at a late period of common cholera; and which is remarkable enough for medical consideration, especially, since it is on the authority of Hoffman.‡ A soldier, 40 years of age, after violent passion, drank a large quantity of *must*, and was exposed to a moist air. He complained of *sundry premonitory* symptoms, followed by cholera in a very violent degree. Stomachics, anodynes, tonics, and diluents were given in vain. On the 4th day, exhausted beyond hopes of recovery, the patient took several large draughts (instinctively no doubt) of cold water, ("aquam frigidam, camque succes-

* It may be asked how it could be otherwise, seeing that the production of animal heat seems absolutely suspended? whence then could this *melted ice* become warm?—Is it not, under all existing circumstances, rather an inappropriate drink, at least in large amount? It does not, it is true, add fuel to the fire—but it assuredly must co-operate, in rendering still colder, the internal organs!

† What was the practice of some of the French physicians?—Was it not to give ice cold drinks in vast amount?—And what was the issue?

‡ *Systema Medicinæ rationalis*—vol. C, p. 513, 4to.

sivis haustibus, magna assumebat quantitate.")—At first, it ran off through the intestines with great pain, which abating, the evacuations became less frequent; the skin, before dry and parched, grew softer and moister, and by degrees, sleep, convalescence and health returned.—A knowledge how far this aqueous treatment might answer in the "pestilential Cholera," seems yet a desideratum. It would nevertheless appear, that Nature *instinctively* calls for fluids, to supply the vacuity caused by such large evacuations, and to give liquidity to those remaining. Sydenham, as a minister of Nature, pursued this indication, by using largely, chicken water; and most physicians have adopted his plan in a greater or less degree, and perhaps, all with different views as to the ratio medendi of treatment.—He, however, enforces it strongly, in order "*partly* to evacuate and partly to dilute" a certain sharp humour to which he attributes the disease. In his account of the Cholera of 1669, at the beginning of August, (which, like that in your city, scarcely reached to September,) though more epidemic than he had ever known it before, he tells us that by this method "which I found out and experienced many years ago, I have always conquered this disease."—"Let a chicken (says he) be boiled in about *three gallons* of spring water, so that the liquor may taste of the flesh; and let several large draughts of it be drank *warm*, or for want of it, posset drink. At the same time a large quantity of the same is to be given at several times successively, by way of glyster, till the whole be taken in and discharged by vomiting and stool," &c.—"When this business is over, which requires three or four hours, an opiate completes the cure."—Now, considering well these remarks of this enlightened practitioner, in which his great cotemporary Moreton coincides, together with the above mentioned case from Hoffman, we may ask again, why the gratification of drinking should be moderated; or how we can draw the exact line of moderation and excess? We find no data by which to judge wherein this "moderation consists.—You employ however, chicken water and beef tea as the "proper nourishment in convalescence"—and no doubt it is then very essential to diminish the *spissitude of the blood*. If, as is the fact, it is often of a molasses or tar-like consistence in this disease, there must be apparently some *innate* power of change in itself, independently of any connexion with the solids. Having no union with them by means of nerves, it is apparently only on, and by, the mere coats of the vessels (nearly insensible) that a mechanical influence can ensue.—Any chemical or vital change in it, would appear dependent therefore on the intrinsic character of the blood itself.—Your enmity to Humoralism in every shape and degree is well established, but this doctrine must prevail;—nay, indeed, I think I see already a disposition in the medical profession to strike the *exclusive* flag of Solidism; and I would therefore urge you to come

over to the good old cause, without further delay.—What emboldens me to hazard this opinion and advice is, that I find the following remarks in your *Therapeutics*, (vol. 1, p. 45, 1st ed.) “No substance in its active state does reach the circulation, since experiments have shewn, that a *few drops even of the mildest fluid*, as milk or mucilage, oil or pus, *cannot be injected* into the blood-vessels, without occasioning the most fatal consequences.”—Here you subjoin a *note*, by which we are informed, that in conjunction with Dr. Lee in 1799, you “instituted a series of experiments, with a view of ascertaining the effects of certain substances, when injected into the blood-vessels. All the articles enumerated above, were tried in succession, together with some others of an acrid and stimulating nature, on dogs and cats, the animals selected for the purpose. But, diversified as these substances are in their properties, we could discern no material difference in their effects, the whole seeming to act merely as extraneous matter in *error loci*, producing at first, great distress* to the animal, as was indicated by its movements and cries, followed by difficult panting respiration, vomiting and purging, (proh Jupiter! a cholera morbus itself!) nervous tremors, convulsions and death.”—This quotation appears in the subsequent editions of your work, in the text itself (3d ed. p. 70,) and other experiments are adduced in support, though very partially selected for specific purposes; as any one may satisfy himself by referring to *some others* of the experiments, &c. of Dr. Seybert, than what are adverted to in your note.—You go on however, to shew that, (*as now*) you confide in no experiments or observations except your own, not even in those of Sir Everard Home, which you are aware, “lead to a different conclusion.”—“Confiding, however, in the accuracy of *our own observations*, I must in the present state of the question, still maintain, *without the slightest qualification*, the position I have assumed!”—What a position is thus voluntarily assumed, in order to maintain without any qualification, that all articles introduced directly into the blood prove fatal!—Nay, you go so far as to say (p. 78, 3d ed.) that you “have only further to remark, that it is not a little extraordinary, and surely betrays the utmost distrust of its verity, that a *practical application has never been made* of the opposite doctrine. Even the most zealous of its disciples do not venture to introduce medicines direct-

* It is worthy of remark here, that at p. 77 of the same edition, in which you thus announce the sufferings of the animals—when endeavouring to oppose the deductions drawn from the *totally* opposite results of experiments by Drs. Lawrence, Coates and Harlan, you say in a Note, that “waiving other objections, conclusions derived from experiments in which the animal is subjected to the most excruciating torture, as was the case here, can never be brought forward to illustrate a healthy process”!!—Consistency is always desirable—especially in scientific researches.

ly into the blood. In vain they have been challenged* to *subject their notions* to this decisive test,—and, while representing the blood-vessels as mere common sewers to the system, *have shrunk* from the proposition of having *a few drops* of the blandest fluid thrown into the circulation. Where is the difference of an active substance passing by the thoracic duct, and the injection of it in the *same quantity* (!) at once into the vessels?—Had you not, my dear Sir, in various places, led us to depend on your “very extensive researches,” (p. 53, 1st ed.) I should have been induced to ask, what authors you really have searched? I know in another work of yours† (on Croup) you say, that “*turning over one of the earliest volumes of the Transactions of the Royal Society of London, I find a very distinct account of the disease, illustrated by dissections.*”—As usual Sir, your references are of that general character, that nobody can find the fact referred to, at least with less trouble than that of hunting up a needle in a bundle of hay! If you had mentioned this early volume and page, we might have referred to it; and, as connected with the subject under consideration, perhaps we might have discovered *fifty instances, in that or some other early volume* of the same work, of medicated articles thrown into the blood-vessels, without a fatal issue.—Nay, for upwards of 200 years back, we may find experiments of nearly a similar character, and down to the present moment of inquiry: which, if doubted, I will give chapter and verse for; and I will introduce only one in a note below.‡

The purport of all these remarks on the subject of injections into the blood-vessels, is, as may be perceived, to point out your inveterate hostility to such a mode of practice,—as dangerous and fatal in the extreme; such Sir is your conviction, in the accuracy of your own observations, from the period of 1799, when your experiments were first instituted, down to the last editions of your *Therapeutics*.—How then, (unless admitting that you have become a convert to Humoralism,) could you, with such powerful reasons to deter from such a practice, permit the “injections of *brandy* and water into the veins” of the second person admitted into your hospital, as re-

* By whom? and where? and when? we here explicitly ask, of the learned professor,

† *Philos. Jour. of Med. & Phys. Sci.* vol. 1, p. 299.

‡ *Lausoni*, in the 1st vol. of his works, p. 460, 4to ed. Lausanne of 1738, states his having thrown into the veins of a mangy dog, an appropriate liquor; which produced frequent vomiting—after which the animal eat, and in 15 days was completely cured.—He then adds, “Unde hanc operationem difficilem quidem esse, ac ferè periculosam prima facie, asserendum est, sed non absurdam, non impossibilem, non fugiendam, ut multi aiunt, putantes remedia omnia, quæ apud eos non sunt in usu, esse absurda, ac impossibilia; novitates enim fugientes isti, et jurantes in verba magistri, calcata ab aliis solummodo vestigia calcant, et more pœcudum eunt quo igitur, non quo eundem.”—Now, the above experiment he states as made in 1682—or 110 years before those performed by you and Dr. Lee.

lated by your assistant, at p. 534, of the *August Medical Journal*?—Unless a change of opinion had taken place in your mind, contrary to those former experiments you have mentioned, I cannot perceive how you could *consistently, or conscientiously*, allow of such a practice; and I therefore hail the fact, as the first step in your retreat from your former “position.”—!—I would moreover very respectfully refer to the experiments of Majendie, Home, Hunter, Orfila, and many other writers of the latter part of the last century, on the subject of injecting articles into the blood-vessels. With such a mass of facts, (perhaps we might actually collect a thousand of them,) shall your observations and experiments, *whilst yet a student*, nullify the observations and experiments of those Nestors of research? What great faith would you now afford, to the experiments of most of your students?—Who then, but must be ready among our votaries of science, both young and old, to cheer you on your return to the right path.

“Thus I have hastily laid before you an outline of *my mode* of managing this terrible disease. It may be observed that with scarcely an exception, it is depletory and evacuant. Deluded by false appearances of debility in the disease, and still more by the weight of authority, I adopted when it first broke out among us, in common with my medical friends, a course of practice in conformity with such an impression; and most disastrous was the issue. Nearly every patient, amounting to five or six, died.”

This disease being one of rarely more than *nine hours* duration; the “*nonum prematur in annum*” of Horace, could scarcely be considered as applicable to your letter, intended as it was for immediate utility. We cannot here, however, omit to state the conviction we have, that the advice of the Roman poet, would well apply to many of our hasty penmen, whose thoughts outran their judgments; and who consequently thrust hastily forward into public notice, their crude and ill-digested notions!

The words *my mode*, in this sentence, of course imply your unqualified claim to it.—It appears nevertheless, that one of your assistants also claims it! “Before concluding these remarks, I will (says he) insert from the day book, the comparative results of the *practice pursued, under the direction of the physician-in-chief, in the hospital, prior to the adoption of my practice and of that which I introduced*,” &c.*—As this practice seems nearly like that you have stated in your letter as your own, we must here ask, Is it yours, or that of your assistant? Surely both do not mean to vaunt of it as theirs?—Let us fully understand what each one claims; that we may know exactly what that practice is, to which

* *Med. Jour.* August, p. 541.

you give an "unqualified preference," and which you call *your own plan*, although not meaning *to vaunt* of its success!

Why call this disease by the opprobrious name of *terrible*, when you so quickly stumbled on that improved plan, by which it became "more tractable than the yellow fever," &c.—and over which a "triumph" was "pretty certain"? To be sure, the value of the plan is diminished, by restricting it to cases that are "not too far advanced"! The difficulty would indeed seem to be, to cure when it was *too far* advanced; *e. g.*: an articulo mortis patient! Unless it can do this, and is only adapted for the former cases, few will see much propriety in singing pæans to its perfection!—As to its being, "with scarcely an exception, depletory and evacuant," it has been shewn to be in this respect, the same that was long since adopted in India; that is at least full 15 years ago; and that it may even be doubted, whether any writers (before fixed principles had been abandoned,) had actually deviated from that plan of treatment; although they may have differed from each other, as to the most appropriate means of depleting, or evacuating the system, either at large, or in its individual parts.

Although Sir, you confess that *you* were "deluded by *false* appearances of debility," permit me to say they were not false! It was the inductions too hastily drawn from those "appearances" that were false! Take any individual symptom you have reckoned as amongst the appearances that constitute the disease, and say, being part and parcel of it, whether it could be false? Do you suppose Nature was playing the fool with you, in one of her frolicsome moods, and laughed in her sleeve at seeing you, the dupe of "false appearances"?—you, whose duty it is, as a shining light, to guard the rising members of our profession, from such delusions!—No Sir! the appearances, so far from false, could not have shewn themselves otherwise; and such may be proved, alike from "the symptoms, the phenomena on dissection, and the mode of cure."—Nature formed their connexion; and it is by seeing them through a distorted medium alone, that by any possibility they could be called false!—But Sir, since you tacitly acknowledge the rapid transition of your opinions and principles of twenty years' teaching; and a complete abandonment of them, to adopt those meretricious impressions, that proved so fatal; how can we be assured of your continuance in the principles to which you have again returned? How be assured that the principles so long taught, are not equally the delusions of false appearances; and that the inspection of the few additional cases which your hospital presented, has not promoted a premature conviction? If we compare the weekly average of your hospital with that of others, we find no vast superiority of practice, even after you had returned to former principles and well tried remedies; certainly nothing deserving of such extravagant affirmations!

—The mortality, connected with the primary mode of practice, is indeed “disastrous” in the extreme!—and by the addition of “*different hospitals*,” in your *corrected* copy, I perceive it is wished to divide with them, the *disastrous issue* you have announced.—The tables annexed, will Sir, set you right in this particular; and shew, whatever may have been the practice and mortality in those other hospitals, yet this disastrous issue of “nearly every patient, amounting to five or six,” was your own entire bill of mortality.

Here I find a concession, my dear Sir, that I should scarcely have expected, viz. that you were deluded, not only by the false appearances above mentioned, but likewise and still more by “*the weight of authority*.”—This astonishes me the more, since I perceive you have abjured elsewhere all other but your own. Who could it be that has thus, by the weight of his authority deluded you to swear “in verba magistri”? or, how could one, who decides only by “actual observation and experience” on the “nature and treatment” of the disease, (see 2nd sentence of the letter,) admit, that he was capable of being deluded on the subject of a practice, which he had for 20 years taught in his lectures!!! The self-experience then, of one period, seems thus controverted by the self-same experience of another! How then can we trust in the continuance of that which is now upheld?

We regret to find, that the delusion was carried so far by some practitioners, dismayed by the “*fearful character*” of the disease, as to abandon their principles and well tried remedies, and “to seek a resource in specifics and nostrums”!

“The prominent indications seemed to call for heat to the surface, and the internal exhibition of the diffusible excitants. Every variety of bath, hot water, vapour, heated air and topical applications of hot sand, or oats, or salt, &c. were used, and also frictions with the spirits of turpentine alone, or united with camphorated mercurial ointment, and other articles. Brandy, ether, camphor, vol. alkali, &c. &c. were in succession tried, and the whole of these means with no other effect than an inconceivable exasperation. The suffering indeed, induced, was as great as I have ever witnessed from the application of any remedial process. No practical lesson is more important than that in the cure of this disease, all such appliances and medicines are mischievous, till evacuations are premised, and then to be most discreetly directed.”

The first part of the indications, was afterwards found to be better answered by evacuations, than by stimuli, although they “*seem-*

ed to call" for them.—The indication then, it seems was, after all, real, and not false in appearance. It was a mistake made by a delusion; and by which the false appearances of debility assumed the characteristics of an oppressed system, which required to be relieved by evacuations!—Permit me however, to inquire, to what point of time in the disease, the "prominent indications" alluded to, are referrible? for, unless informed of this, it is obvious, that, like a revolving wheel, new points become prominent, and other indications present themselves.—In the use of "every variety of bath," &c. &c. the inertness or inutility of many articles, is well depicted: and, like the brandy, &c. mentioned as successively tried, being productive of "no other effect, than an inconceivable exasperation," served to rectify that misapprehension of the real indications; and led, no doubt, to that salutary return to principles and practice, from which judgment had been led by an ignis fatuus; and thereby to an escape of further witnessing the unexampled sufferings, so feelingly noticed.—How truly distressing must it have been, to consider, that the important practical lesson adverted to, viz. that all those stimulating "appliances and medicines are mischievous till evacuations are premised," was not attained, without the conviction, that the suffering that presented itself, was not necessarily nor essentially connected with the disease; but arose out of a dereliction of principles and well tried remedies! But for this, both here and elsewhere, the disease would have been immediately triumphed over; and the world would not now have to lament the fate of millions of victims, and the destitution of orphan children.

"It were easy to acquaint you with divers other methods of treating this epidemic, or to enumerate a number of special remedies that have been proposed. Dismayed, as it were, by the fearful character of the disease, practitioners have been too prone in its treatment, to abandon their principles and well tried remedies, in analogous cases, to seek a resource in specifics and nostrums."

This would have been indeed, the *multum in parvo*, and greatly is its omission to be regretted.—Perhaps however, the epitome, or tolerable synopsis, of Dr. Johnson in the *April* number of the Med. Chir. Review, (and consequently in *Philadelphia*, long before the

Cholera broke out there) will supply the place of such an important document as would have thus been presented.—I must refer nevertheless, to the extract as given in the American Journal of Medical Science for August, 1832, p. 536 *—in which, singular to say, we find the bleeding and calomel practice, at the very head of the list!—and from that list we find moreover, that every plan adopted here, had a *parentage of foreign origin*! It is certainly just as easy to say *my*, as *your* practice; and if people will not read the various treatises themselves; but are satisfied with the *summary* of partial or partizan Reviews, why, it follows that they may easily be misled; and ignorantly believe all that is thrust upon them.—In regard Sir, to the “*special remedies*” that you say have been proposed; it might have been a subject of interest to those future generations of our profession, whom your letter may reach, to have discovered, that amongst them was to be found, a remedy that owes much of its high character to your own former recommendation of it, in other diseases; although, as in the present case of Cholera,

* “In proceeding now to speak of the treatment, we cannot give a better idea of what has hitherto been done, than by the following extract.”

‘In each country where this disease has appeared, we find a very different and opposite treatment recommended, as one proved by experience to be the best. In *India*, *bleeding*, *calomel*, and *opium*, were the favourable remedies. In *Russia*, a practice as inert as a few grains of the sub-nitrate of bismuth in frequently repeated doses. In *England*, the *mustard emetic*. Again, in different countries, remedies contradictory, as *bleeding* and *transfusion*, are proposed and made use of in full confidence. Heat applied to the body in every form, as well as the cold affusion. Drinks altogether denied, or ordered in unlimited quantity, both hot and cold. They have attempted to restrain purging and vomiting by the most powerful narcotics, enormous doses of opium; and they have also encouraged them by *various emetics* and *purgatives*. Some try to allay the irritability of the mucous membrane; others goad it with the most powerful stimulants, ardent spirits, and ammonia. Others place their reliance chiefly on the mild alkalis, soda, and magnesia, to neutralize some imaginary agent; and, again, some indication has been found for acids; whilst many have trusted a trifling carminative, as essence of mint, or cajuput oil, to combat these alarming symptoms;—illustrating the remark of Sir William Crichton; ‘it is a most melancholy confession, but one not the less true, that after Cholera has spread its devastations from Ceylon to Archangel, from Oranburg to Berlin, we are almost as far from a rational *methodus medendi* as we were when it first appeared on the banks of the Ganges.’

“Here is a summary of medical authorities upon the best mode of treating cholera. Uncertain as has always been the science of medicine, our records do not show any thing quite so extravagant as this—for although different systems of treatment and different theories have, in succession, each had its turn, it is something new to see so many put forth at one time, and each one supported by such high authority.”

* *Johnson's Med. Chir. Rev.* April, 1832.

the opinions at first * pronounced, as formed on self-observation, were subsequently,† completely reversed by your candid avowal, that you had "overrated the value" of the Panacea of Swaim!! and that cases are in your possession, "eminently calculated to alarm the public on this subject." Such perpetual change of opinion in medical science, must, I fear, tend greatly to shake the confidence of the public in the certainty of that science; and not less, lead to the

* Feb. 16, 1823.

† Septem. 29, 1827.

CERTIFICATE of N. Chapman,
Professor of the Institutes and Prac-
tice of Physic, and Clinical Practice,
in the University of Pennsylvania,
President of the Academy of Medi-
cine of Philadelphia, &c. &c.

"I have within the last two years had an opportunity of seeing several cases of very inveterate Ulcers, which, having resisted previously the regular modes of treatment, were healed by the use of Mr. Swaim's Panacea; and I do believe, from what I have seen, that it will prove an important remedy in scrofulous, venereal and mercurial diseases.

N. CHAPMAN, M. D.

"*Phila. Feb. 16, 1823.*"

☞ See the public Newspapers of the above date;—or, a small "Treatise on Swaim's Panacea": Philadelphia, Printed by J. Maxwell, 1825.

Extracted from the Report of the Committee appointed, &c. See *Periscope*—p. 483 of *Am. Jour. of Med. Sci.* No. 2, Feb. 1828.

Letter from Professor Chapman.

"Excepting 'Swaim's Panacea,' I have no knowledge of any of the nostrums to which you allude in your communication to me. Farly in the history of that article, I was induced to employ it, as well from professional as common report in favour of its efficacy, and was well pleased at the result in several cases. But more extensive experience with it, soon convinced me that I had overrated its value, and for a long period I have entirely ceased to prescribe it.

"As to its composition, I have satisfied myself, and by no equivocal evidence, that it essentially consists of a saturated decoction of sarsaparilla with corrosive sublimate, and that it is an inferior preparation to the syrup de Cuisinier, principally constituted of these two ingredients, and which is now so much used in the practice of this city.

"It were easy to point out, and indeed to demonstrate, the great mischief which has resulted from the indiscriminate employment of this nostrum. and I am in possession of not a few cases, which, if you wish them, are at your service, eminently calculated to alarm the public on this subject.

"(Signed) N. CHAPMAN.

"*Philada. Sep. 29, 1827.*"

It is not our business to settle the discrepancy of the above opposing and conflicting testimonials from the same pen; both equally dependent on observation and self-experience.—We can only remark, that in complete opposition to this last extract, we find the papers of the Union still teeming with the above original Certificate of recommendation!—How is it that a vendor of nostrums should thus have it in his power, to drag triumphantly at his chariot wheels, a regular practitioner, so closely connected with the first medical school in the Union?—proh pudor!—and which of the above statements are the members of the profession to accredit, seeing, as it appears on their very face, that each bears the stamp of experience? Can we have a right to suppose, that experience can be so delusive; for if so, well may it be asked, wherein does medical experience consist?

inquiry among the members of our profession itself, as to what confidence may be placed in opinions so frequently at variance!—As to the special remedies enumerated in the newspapers, we find them in abundance, from Swaim's Panacea and Vermifuge, down to Burgundy pitch plasters, and three drops of camphorated tincture!—In vain has the hydra-head of quackery combined with the *regular* train-bands of the profession. Cholera still slays its thousands; the *cures* are scarcely to be numbered by hundreds.—No wonder that “dismayed” practitioners, terrified by its “fearful character,” should be “prone in its treatment, to abandon their principles”!—May it not be safely affirmed, of *such practitioners*, that they were a most abandoned set? thus to leave the luminous principles they had imbibed; and hastily abandon principles and well tried remedies, in order to seek resource in specifics and nostrums!—But Sir, have you not been *too general* in this sweeping clause of condemnation? It is to be hoped, that at least ten might be found in your city, to ward off the reproach! Alas! the stigma now attaches to all, from your unfortunate omission of the names of the culprits.—You should have hung them up by name, for the slow unerring finger of scorn to point at!—I trust you have too hastily advanced a sarcasm on your professional brethren, that they by no means deserve; and that the word “dismay,” finds no place in their vocabulary! No doubt, a few deserters might be found from the forlorn hope of cholera practice, and I have indeed heard of such, whose dismay and alarm at the first onset of the disease was so excessive, that it was but the toss of a copper, they had not deserted in the period of calamity; I hope sincerely they were few in number—and should this pamphlet reach a second edition, I will promise, if you will forward the names of such delinquents, that I will give them a prominent station, on your authority!

“I do not mean to vaunt of *their* success, but on a fair comparison of all that I have seen attempted, I am led to an unqualified preference of *my own plan*. It cannot be charged with being tentative or empirical; it is deduced from established views of pathology and therapeutics, and is sanctioned in most of its features by the lengthened and concurrent experience of the authoritative writers on the disease in India.”

I am aware Sir, from the *corrected* copy of the letter, that the words *their*, and *my own*, are altered to *my*, and *this*. I have already shewn that your claim is conspicuously held forth in many places; and here also we find it confirmed, by which ever reading you chuse to call correct! The claim, we have seen, is contested by your Aid: and it remains to see how this claim can be settled to your mutual satisfaction! You do indeed, in some measure decline

the honour of paternity to the plan, by immediately stating, that it (your plan) "cannot be charged with being *tentative* or empirical;" it being "deduced from established views of pathology and therapeutics, and is *sanctioned in most of its features* by the lengthened and concurrent experience of the authoritative writers on the disease in India."—I should have presumed you were satisfied in giving it to the writers you notice; had you not restricted it to "most of its features," thereby giving us to understand, that *all but* "the most" of its features, belong to you.—Now, I would desire you to present *any* individual feature, which they have not announced! Of *their* experience, you may well say "*lengthened*," for it *extends* to at least 15 years, so far as it relates to the Indian writers; whilst *yours*, is embraced in the period from about the 1st to the 18th of August!—*Concurrent* therefore, will scarcely apply! Now, the first portion of this prolonged period of nearly three weeks was obviously taken up by those plans of treatment that were found so fatal, and which you so happily abandoned!—With the admission made throughout your letter, we must be allowed to add, that, *if not empirical*, it was assuredly *tentative*; at least I apprehend it will be so considered by 99 of 100 of your medical readers. If you ask me why I think so, in opposition to your self-assurances, I shall very candidly reply, that admitting as you have done, the "fearful character of the disease," unless it is absolutely demonstrated that you were not among those practitioners, who, from dismay, abandoned principles and well tried remedies in its treatment; I cannot for my life view it in any other way!—Although I should be glad to find that you could divert from yourself, this obvious deduction from your own words; yet I must add, that the postscript to your amended copy, positively strengthens the presumption; since you there tell us, that you had taught "this very practice, by emetics, calomel, bleeding, &c." for 20 years in your lectures?—Why, *if then most appropriate*, did you doubt at all of their absolute necessity in the cases before you?—Why leave that practice for one you soon discovered to be so fatal?—You refer us to a "tolerable synopsis" in your work on the *Materia Medica*; which synopsis I have in vain looked for!—for, your reference is altogether general, and does not point to any particular part. Few probably will take the trouble that I have done to hunt it up; but will be satisfied to take it upon trust, on the authority of your assertion; and will credit you for a synopsis which I have *not* been able to discover. To be sure, I may not have looked in the right place; but, since in your postscript you particularly make emetics, calomel and bleeding precede the &c., so I have limited my researches to those points, and have left the &c. for further investigation.—I shall merely add, that I, of choice selected your *first* edition, as being nearer to the actual period you advert to; although 20 years

are not yet fully accomplished, since you were raised to a chair, that you appear now to think of little importance in a medical school. As regards emetics, Sir, I find no notice of their use in Cholera, when you are treating of the "bowel affections," p. 88, vol. I. This disease is not even mentioned when treating of Ipecacuanha, and its particular employment in bowel complaints.—In short, so far as I can see, this disease is unnoticed in the whole discussion of emetics; except as incidentally excited artificially, by tartar emetic, to remove poison from the stomach.—In speaking of calomel, p. 311, vol. II., and mentioning it expressly in the "bowel affections"—and its employment in the E. Indies by Yates and Maclean in Dysentery; yet you are not led to notice its use in Cholera!—Where Sir, is it then to be found?—As for bleeding, important as it is, in a remedial point of view, it finds no *particular* notice from you. Cursorily indeed, it appears in your pages; but I have not found any especial connexion of it with Cholera; and hence, I must very earnestly request you to point out the part, which I may have unfortunately overlooked; and which may also be the case with others.—We shall thus be able to compare the respective claims of yourself and of the E. India practitioners: for although you seem to take precedence of them by your remarks, I cannot find that any of them have looked to your writings as the source of their treatment during the period of 15 years.

"Many may be cured with it, and some will sink under the force of the attack in despite of your efforts. The case not being too far advanced, a triumph over the disease is pretty certain.—Cholera is, on the whole, more tractable than yellow fever, or the winter pestilence, which devastated our country during the late war.

"Ever, my dear Sir, yours most truly.

"N. CHAPMAN.

"Philadelphia, August 18, 1832.

"To Dr. WM. BRADLEY TYLER, Frederick, Md."

This sir, is precisely the amount of information we derive from the admirable writings of the E. India and other practical men, during a period of 15 years—or 780 weeks;—to which, your portion of three weeks' hospital practice is as the 260th part, whilst your 37 patients to the millions recorded must now appear as a drop in the bucket!—It is probable Dr. Tyler was well acquainted with the writers adverted to, if not directly, at least from the periodicals which have annually recorded them. How truly happy must it have made you, and what a source of felicitation from your correspondent, that you so fortunately discovered the important fact announced in the antipenult sentence of your letter! viz. that if "*not too far advanced, a triumph over the disease is pretty certain*"!! My dear

Sir, pray inform your readers, if it is the triumph of Nature or of Art—of principles re-adopted, or what?—I think I see Dr. Tyler proclaiming this important truism, as one, with which the writings he had perused, had made him familiar! Nay, with which his daily experience in every other disease, must have informed him! We entreat you to acquaint the profession, whereby we may exactly distinguish the line by which we reasonably mark a triumph, if triumph we can call it, when “not too far advanced.”—Can you inform us also how to triumph over the disease when it is “too far advanced”—or is this beyond the reach of all scientific principles and practice?—In which of these two different stages of Cholera is it that it may be deemed “on the whole, more tractable than yellow fever, or the winter pestilence?—for if this is really true, why was Dr. Tyler and the public, to be terrified by the appalling influence of your descriptive pen? *

And now Sir, in bidding you farewell, allow me to ask, with the utmost respect, and with absolute sincerity of intention, wherein has your letter been calculated to be useful, either in theory or practice, beyond what has long been known to every one who kept pace with medical science; and had not thought it necessary from undue dismay, to depart from long established principles, and plunge into a sea of doubt and despondency. I should not have deemed it necessary to remark on the letter at all, had it not been, for the very general and prevalent report at the time, that Dr. Chapman had discovered a new method of treating the disease, which had divested it of much of its hazard;—and I was therefore led to examine it with a greater attention, than I should otherwise have considered it entitled to.—

With great respect, your well wisher—

AN INQUIRER.

* May we be permitted to ask, if the mortality of yellow fever or winter pestilence, has ever equalled in any hands, that of Cholera? we will not say at the fatal period of the first few days; but at and after the happy period of more felicitous practice.

THE END.

ERRATUM.

Page 18, line 12, for “and” read *et*.